JOURNAL

OF THE

FRANKLIN INSTITUTE

OF THE STATE OF PENNSYLVANIA,

FOR THE PROMOTION OF THE MECHANIC ARTS.

VOL. CXXII. DECEMBER, 1886. No. 6.

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THE ELECTRICAL EXHIBITION AND PURE RESEARCH.

BY M. B. SNYDER, Professor of Astronomy, Central High School, Phila.

[*A Lecture delivered before the* FRANKLIN INSTITUTE, *February 8, 1886.*]

When requested to develop before the INSTITUTE, some topic relative to the late Electrical Exhibition, it occurred to me that there was at least one line of reflection it were well to insist on, before the memory of that brilliant display should fade too deeply. There were those who had reported upon the uses and peculiarities of the special exhibits. There had been full account taken, as well of the praiseworthy inventive and mechanical skill displayed as of the immediate practical bearing of the several inventions. There had even been attempted practical tests which should haply add to the commercial recognition and credit of the inventor.

But what should be said if in our Exhibition, we failed to recognize anything more than the genius which presides over mechanisms and patented devices; if, through thoughtlessness or neglect, we ignored that great body of unregistered exhibitors, who, by their researches alone, had given silent though effective

tribute. It seemed to me that even America, with all its worship of the immediately practical, and with all its bold faith in haphazard progress, could well afford, as for a moment it cast a last look upon its first great electro-technical exhibition, to do homage to those far-seeing minds, who in this field had made invention possible and progress a necessity.

I propose then that the thread of our reflections shall be the dependence of the Electrical Exhibition upon pure research. And, to prevent misapprehension, I desire that the words "pure research" should be understood to imply that class of scientific work which means investigation for the sake of the beauty of truth and fact, though the applications and the mercenary rewards be never so remote; that holds the advancement of the borders of knowledge under the impulse of an unmeasured faith in the eventual advantage to be its highest, noblest aim. It should, however, not be inferred that antagonism to the so-called "practical science" is intended, except in so far as that name is a cloak for empiricism. Indeed, it would be folly at this stage in the history of any science to moot a distinction between the workers in theory and the workers in application, seeing that it is now well understood that the very highest efforts in each field are only attainable by a just recognition of the claims of the other. We know well that one theory may comprehend a thousand applications and that one appliance may demand a score of theories. Research and practical application are infinite debtors, the one to the other. And yet the habitual neglect of the claims of the former in our new civilization, is sufficient reason for limiting our attention to research as having so richly contributed to our present practice in electricity.

In addition, however, to the motives already indicated, I should mention at least two others as urging to this line of thought. The first is: The blank discouragement and almost insuperable difficulties encountered by the American student, who may aim to further research for its own sake; the second: the pressing necessity for establishing in our midst foundations whose chief aim shall be the advancement of physical research, and the indoctrination of its results into the public mind. To these points, I shall now and again refer.

For if we could show but a little more clearly how the great practical operations exhibited, are the direct result of the past

study of experimental and theoretical science; if we could show that all the great inventions presented to our view drew their very existence from the paitent and faithful investigations of men engaged in pure reseach, might we not entertain the fond hope that some more decided, some more earnest movement should be initiated to further that scientific spirit in our midst?

It would be useless to disguise the difficulty of making a statement of this dependence of the exhibited results upon pure science, that should deeply affect every mind. To the majority of spectators at the Exhibition, I have no doubt that the prominent impression was the beautiful spectacle realized by the inventor's skill. At most, the immediate practical result, was all this type of mind cared to appreciate. They may have been dazed and somewhat puzzled, but, it is to be feared, that to them the presence of the real presiding genius was never clearly disclosed. There are, however, others who glance through the great display with different eyes. To these are present the long array of experiments and theoretical investigations at the basis of many a single practical result. And again they recall a single experiment, which means ten thousand dynamos of a hundred different types. Let us then join this group as it studies the Exhibition and gather at least a few glimpses of the real origin of the important industrial display.

Our attention, as we enter, is for a moment arrested by the title: "The *International* Electrical Exhibition of the FRANKLIN INSTITUTE." Now the warmest admirers of the Exhibition admit that there was comparatively little in the way of foreign exhibits to warrant the title international. The quick succession in which such exhibitions had been held in Europe, and the consequent multiplied expense to exhibitors was undoubtedly the factor which prevented the full realization of the plan of the INSTITUTE. And yet, from a philosophical standpoint, the Exhibition could not be less than international. We shall, indeed, have to say that nearly all the great spirits in physical research, to whom we have to refer as contributors to the result, were nurtured by the European nations; and that it is to the enlightened encouragement of research abroad that this Exhibition owes most.

As we now glance over the character of the electrical display and note the deftness with which this strange form of energy

serves the purposes of our modern life—as we find electricity no more a mere plaything in the hands of a few scientists, but now the willing servant to a thousand and one earnest uses, we cannot but mark the severe contrast between this result and the unpromising beginnings.

Instinctively our imagination turns back to that early American time, when in or near the self-same locality there was to have been held another electrical exhibition. Strangely enough, we call this the Electrical Exhibition of the FRANKLIN INSTITUTE, while the other was an exhibition given by Benjamin Franklin himself. The marvellous demonstrations in electricity made at that exhibition must indeed have stirred the reflective minds of early Philadelphia as deeply as the wonders of our Exhibition could have affected any of us.

It seems that in a mild spirit of revenge upon the great difficulties encountered in developing anything of immediate use from his electrical experiments, and as a sort of semi-ludicrous, semi-earnest challenge to those who doubted the eventual utility of such high scientific pastimes, Franklin wrote of that earliest American electrical exhibition, the following prospectus which I shall reproduce with his own emphasis:

"Chagrined a little that we have been hitherto able to produce nothing in this way of use to mankind; and the hot weather coming on, when electrical experiments are not so agreeable, it is proposed to put an end to them for this season, somewhat humorously in a party of pleasure, on the banks of *Schuylkill*. Spirits at the same time, are to be fired by a spark sent from side to side through the river, without any other conductor than the water; an experiment which we some time since performed, to the amazement of many. A turkey is to be killed for our dinner by the *electrical shock*, and roasted by the *electrical jack*, before a fire kindled by the *electrified bottle:* when the healths of all the famous electricians in England, Holland, France, and Germany are to be drank in *electrified bumpers*, under the discharge of guns from the *electrical battery*."

We are happy to say that Franklin's dream of something "of use to mankind" from electrical research was fully realized at the recent Exhibition, held but a little more than a century after that of his own.

What a glowing picture should we have had from the ready

pen of the old statesman, could he, by some magic of the imagination, have been invited into the ideal studio of our modern exhibiting electrician. There he should have found an apartment decorated on every hand with the beautiful results of the electro-metallurgic art, furnished with tables bearing a wealth of electro-plate, with books, fine pictures and rare scientific drawings reproduced from electrotypes, with clocks strangely moved and controlled by electricity, with electro-chronographs recording to the utmost fraction the time of occurrences a hundred miles distant, with electric signal buttons ready to register both an audible and visible signal at points near or remote, with telephones that allow a conversation to be held at a distance of twenty miles as easily as with one's neighbor, with telegraphic sounders whose brazen hearts thump with messages that have travelled a thousand miles, with a mirror galvanometer whose delicate indications reveal thoughts that have held their tireless course through three thousand miles of ocean cable, with sphygmographs that record the frequency and the varying force of the pulse of the sick; with stock tickers telling the continuous story of the rise and fall in the pulse of the commercial world; with signalling devices that make police and fire departments, as well as a host of messengers, our house servants; with electric lamps shedding the soft radiance of daylight, and serving a score of uses, both useful and artistic; and finally, with electric-motors ready to move all sorts of machinery, or, if we choose to use the studio as a travelling-car, to speedily propel us from town to town by means of the mystical energy that may pass through a simple wire. All this and much more would have contributed to the contrast of the two periods of early experiment and later use.

If we should only regard the visible results, we might perhaps consider the lightning-rods attached to our Exhibition building as the only remaining indication of the early activity of Franklin and Kinnersley. But we must here insist on a deeper insight into the relations of effect with cause in electrical history. Franklin's chief glory consists in the fact that he was among the first to propound an explanation of electrical action; to clearly moot a theory which should thenceforth be used for the furtherance of experiment. If he was among the first to show experimentally "the wonderful effects of pointed bodies, both in *drawing off* and throwing off the electrical

fire," he was also among the first to take a broad survey of the whole problem of loss of electrical charge, and above all to propose to himself such a conception of the action of the "electrical fire" as should be applicable to every form of experiment.

All the way from the sixth century B. C., when Thales rubbed the *elektron* and noted its attractive effect on light bodies, down to the middle of the eighteenth century when Franklin and Symmer first stirred the scientific world by their theories of electricity, we note only, for the most part, isolated experiments. Gilbert, who first distinguished between magnetism and this force which he thence called electricity, had indeed essayed to explain the attraction of electrified bodies by the analogy of the melting together of two drops of water. Otto Guericke had discovered that electrified bodies also suffer repulsion and probably first had seen an electrical spark. Stephen Gray had distinguished between conductors and non-conductors, and had noticed the electrification of bodies by mere presence, by induction. Du Fay had proved that two distinct kinds of electrification might be excited by rubbing different substances, and that the one class of rubbed bodies attracted the other class of rubbed bodies while each repelled its own kind. Van Kleist had discovered what is now known as the Leyden phial, and Van Musschenbroek had made it famous among all the learned. Winkler had set fire to a number of inflammable substances by means of the electric spark, and many more curious results had been obtained. But futile, indeed, would have been these isolated experiments, if Franklin, and Symmer, and Winkler, and Aepinus, and Wilke had not set the world agog by a famous contest over the theory of electrical action.

Experiment was opposed to experiment and explanation to explanation in the sharp battle for truth between the unitarian and the dualistic theories of electricity. What matter if the contest between them has continued even to our own day, and the true nature of electricity be almost as much an enigma now as then. It has been the great unsolved problem that has stimulated research and incidentally brought about discoveries of the first magnitude. We learn here again, that the clear mooting of a scientific theory may have an immense influence on scientific and material progress, in that it provides an aim and suggests a plan for experimentation. Scientific history abounds with examples

which might be cited in illustration of this sort of stimulus to progress. You will allow me to divert your attention to an analogous and notable example in the history of astronomy.

When Copernicus found that the conception of the earth's motion around the sun accorded best with his observations of the positions of the planets, and he announced the bold hypothesis, it was opposed on the ground that if the earth moved in an orbit, the fixed stars should also appear to describe small movements on the heavens; in other words, we should be able to determine the parallax, and consequently the distance of the fixed stars in terms of the earth's distance from the sun. No such movement was observable in the stars, and Copernicus was obliged to assert that the stars were so distant and the displacement due to parallax so small that his measurements could not detect the motions. Here was the challenge to the improvement of the instruments and methods used in observing—a challenge which bore the richest fruit. Copernicus could determine stellar position within several minutes of arc, Tycho Brahé within one minute, Bradley within a second or two; and only when Bessel, in 1836, brought it down to tenths of a second, was the distance of a fixed star actually measured. The final proof of the justice of the Copernican statement concerning the immense distances of the stars was, however, by no means the main acquisition to astronomy. Tycho Brahé had laid the foundation for the discovery of Kepler's Laws, Bradley had discovered aberration and nutation, and astronomy had advanced to the point of refinement in the measuring process that made the discovery of the law of gravitation by Newton possible. But above all, delicate instruments had been invented and rigorous mathematical methods of observing had been devised, which should remain a rich legacy to physical science for all time.

Thus also in electricity were the arts of observation perfected, and the discoveries multiplied by a ceaseless desire to prove the correct conception concerning the nature of the electrical action. On the one hand, electroscopes, electrometers, unit jars and other devices for the qualitative and quantitative measurement of this force were devised by many. On the other hand, it was reserved for Tobias Meyer to suggest and for the great Coulomb to prove that electrical repulsion and attraction followed the rigorous mathematical law of decrease according to the inverse

square of the distance. These men were both able mathematicians, but Coulomb in addition made himself the father of electrical measurement by the invention and shrewd application of the torsion-balance, and you shall find that the great discovery he made, is fundamental to all electrical theory and practice. Oft-times this law of Coulomb is hid by the later deductions and by the nearer rules of practice, but if you go back to fundamental conceptions you finally cast anchor in a little mathematical expression borrowed from that which states the law of gravitation. Thus, in the search for the true nature of electrical action, now an ingenious instrument, now a great law is discovered

It is here to be remarked that although all this was accomplished in electricity, generated by friction, we find scarcely any vestige in our Electrical Exhibition of this method of production. Indeed, I do not remember to have seen there a single instance of this method of producing electricity. And yet there, as constant reminders, stood the old historical machines and there the educational examples of the "balance à torsion" of Coulomb.

If, now, we seek out the sources of the electricity, used in most of the ordinary electrical appliances shown, and used particularly in connection with our devices for communicating thought to a distance, we shall find the voltaic battery silently furnishing a constant supply.

It was in 1790, that Galvani first announced the great discovery which led finally to the development of this new source of electricity. We will not, as has so frequently been done, place this discovery to the account of the mere accident of the twitching of frogs' legs, when touched by metal, as they lay near an electrical machine. That was undoubtedly the occasion of directing Galvani's attention to a new phenomenon; but we must not ignore the fact that Galvani, as a student of medicine, knew something of animal electricity and of electricity in general. For to his interest in animal electricity and to his bias in that direction, we must attribute both his closer study of the occurrence and his theory that the real seat of the electricity was in the nerves of the frog.

It was, however, to Alessandro Volta, that the greater credit was reserved of setting the whole matter in a clear light. Although his first work on this subject bore the title of *Animal Electricity*, he soon directed his attention to the contact of the metals as the

cause of the electrical disturbance. With the method of a trained physicist he ceased to use the frogs' legs as electroscope and showed that still the simple contact of two metals produced an electrical action. You remember the old experiments with two coins of different metals which might either produce an acid taste by contact in the mouth or a flash of light before the eye if it were included in the circuit. With this, Volta's investigation had but just begun. He called attention to the fact that, in this same experiment with the metals in one relation in the mouth the taste was acid, in another relation the taste was that of an alkali, and that moreover the taste *continued* without interruption so long as the contact of the metals continued. This we now recognize as the first glimpse of the chemical action of the electrical current. Intent on clearing up his contact theory of the production of electricity, he studied the effect of contact of various metals, arranged them in a series in which the middle terms might be removed with indifference, and proved that a current could only be produced when a liquid intervened in the series. He discovered the first form of what we now know as the voltaic battery. Everyone knows something of the classical contest between Volta's contact theory and the conception of the animal electricity of Galvani, as well as of that other greater contest between the Volta contact theory and the chemical theory of the battery. It required a Commission of the Institute of France, with such members as Laplace, Coulomb and Biot to give full recognition to the immense services rendered to science by Volta's labors.

It is interesting at this time to note that at least three lines of investigation diverge from the standpoint of Galvani and Volta. Galvani was absorbed in animal electricity, and among the number of experimenters who, with renewed vigor, took up this line of research could be reckoned Von Humboldt, who is said to have inflicted on himself a large and painful wound in order that he might study the relation of electricity to animal tissue under normal conditions. If medical electricity, so fully represented at the Exhibition by a variety of appliances, has any sufficient reason for its existence, it should have profited by these and a long series of similar researches that followed in Galvani's wake. On the other hand, Volta inaugurated a series of the most refined investigations on the potential of contact, and it is probable that no other

electrical question has developed more refined methods of experimentation. Although a brilliant line of physicists, from Volta to Sir William Thomson, discuss and experiment on the question, the mystery is still ours. The third direction given to investigation was, as you know, the chemical one, with its Ritters, its Davys, Becquerels and Faradays.

It would serve no useful purpose to recall the names of a tithe of the chemists and physicists who have step by step developed the voltaic battery as now used. Whether of one fluid or two fluids, of higher electro-motive force or of lower internal resistance, it matters not, the battery is the finished product of an hundred willing workers in the field of pure research.

No one who fails to recall the familiar history of electrical progress can have a just conception of the many and widespread ramifications incident to the introduction of the Voltaic method of generating electricity. We have only to pause before one of those large storage batteries of the Exhibition to be reminded of the early discovery by Ritter of the polarization of the voltaic cell, and the later developments of polarization, particularly in the hands of Planté, into a factor as useful as it was once inimical. This is but an instance, and very briefly stated, of many lines of research suggested by the battery. We mention next the galvanoplastic processes so rich in utility and beauty, as due to the study and use of the battery in electrolytic action, by Faraday. The electrotypes, electro-plated ware, and all sorts of artistic reproductions, seem fully to repay the wearisome investigations in the field of electro-chemistry.

But, in passing to and fro through the Exhibition, we have noticed many appliances which owe their utility to some relation between electricity and magnetism, and Oersted's early experiments with the galvanic battery, and his consequent discovery of a direct relation between a current of electricity and a magnet, can no longer be kept in the background. We here come upon another epoch-making experiment.

It was in the course of preparation for a lecture on "Electricity, Galvanism and Magnetism," in the spring of 1820, that Oersted, as he informs us, first tried the celebrated experiment of the effect of a current of electricity on the magnetic needle. It cannot be said that the discovery was altogether unsuggested by the previous

course of science. Franklin had magnetized needles by the passage of the electric spark, and Oersted himself had remarked the uneasy movement of the magnetic needle during a thunder-storm. It was then the next step to imagine the influence of the electric-current on the needle. Oersted not only exhibited the action of the current on the movable magnetic-needle, and conversely by freely suspending a little battery caused it to move by means of a magnet as though itself a magnet, but he brought out clearly the relation of direction of the current to the deflection of the needle. His little pamphlet on the subject, written in the Latin language, and hence at once accessible to all the learned of other countries produced a profound sensation in scientific circles. Eagerly were the experiments repeated and with astonishing rapidity were new discoveries added. In Germany, Seebeck, Erman, Poggendorf and Schweigger took up the subject; and the very galvanoscope then made by Schweigger and Poggendorf by coiling the wire a number of times about the movable magnet so as to multiply the Oersted effect, is our current detector to-day.

It was, however, in France that the news of this discovery was to be crowned by developments of even greater moment than the original experiments of Oersted. Arago brought the news to Paris and, assisted by his friend Gay Lussac, showed that the electric current not only deflected a needle, but magnetized a steel-rod when it was passed through a spiral wound round the rod. They also showed that a wire included in the galvanic circuit actually played the part of a magnet in attracting iron-filings. You remember how similarly the attendants of some of the heavy-current dynamos at the Exhibition amused us all by showing the power of a simple copper-wire to attract nails, files and such like. That, we suppose, was but a playful recognition on the part of the dynamo of its high-born blood.

To André Marie Ampère, however, belongs the glory, not merely of making a few novel experiments but of so theorizing upon the experiments made, and of so inventing new experiments according to theory, and as groundwork for the development of theory that he was enabled to put in place the very corner-stone of the science of electro-dynamics.

And just here we must be permitted to say that there could be no better commentary on the powers of mathematical training in

determining the best course of progress, than to institute a comparison between the articles of Oersted and of Ampère, as they stand recorded in two successive volumes of the *Annales de Chimie et Physique*, of the year 1820. But for the lucid conceptions of the French mathematician, we might still be speaking of the "electrical conflict" of Oersted. To state a few of Ampère's achievements in electricity: He introduced the convention, which has become permanent, of regarding the direction of the current to be that of the positive electrification. He stated so simple a mnemonic for predicting in what direction the needle will be deflected by a current; that the current always "enters the heels" of the practical electrician, and "flows out at his head," when he "looks upon a needle whose north end is deflected to the left." He found that currents acted either attractively or repulsively on currents, according as those were in the same or in opposite directions. But the most important step taken by him was in introducing an electrical conception of magnetism. Since the needle directed itself at right angles to the current, currents flowing from East to West round the earth must produce its magnetism, while artificial magnets are also to be conceived as formed of molecular currents. The whole question then resolved itself into the simple elements of the action of one current on another. The problem was now one of mathematics and mechanics, and he resolved it with abundant success.

As we leave this eminent contributor to the realities of our Exhibition, we cannot omit the beautiful eulogy Clerk Maxwell wrote, when he himself recast the mathematics of the subject: "The experimental investigations by which Ampère established the laws of the mechanical action between electrical currents, is one of the most brilliant in science. The whole, theory and experiment, seems as if it had leaped full-grown and full-armed from the brain of the 'Newton of electricity.' It is perfect in form and unassailable in accuracy, and it is summed up in a formula from which all the phenomena may be deduced, and which must always remain the cardinal formula of electro-dynamics."

We pass now to the next great step that had to be taken before current electricity would become the facile, intelligent servant of the world. This time, also, it is the able mathematician who, in the person of George Simon Ohm, first discloses the won-

derfully simple relations existing between the conductivity of a circuit and the electricity passing. There is no practical electrician, however ignorant of the fundamental demonstrations, or of the repeated experimental and theoretical verifications, that does not pin implicit faith to Ohm's law. Electro-motive force = current × resistance: is the magical rune now applied in a thousand mystic transformations common to the art and practice of electricity. If space permitted, the line of thought of Ohm's celebrated memoir would be an interesting topic; but we must at least remark that, taking into account the changes in our electrical nomenclature, the very statements of that memoir have now become the familiar conceptions of all who comprehend what is meant by the term "electrical current." Through Ohm, the practice of electricity is also probably a great debtor to no less a mathematician than Fourier; for Ohm himself states how the conceptions Fourier had used in his great work on the theory of heat, were by him transferred to electricity. And we mention this indebtedness in order to be able to hint at the vast obligations due by electrical progress to the prior cultivation of mathematical analysis in other fields. In connection with such an application as that made by Ohm, Fourier's own words seem like a revelation. "Mathematical analysis," says he, "seems to be a faculty of the human mind, destined to supplant the shortness of life and the imperfection of the senses; and what is still more remarkable, it follows the same course in the study of all phenomena; it interprets them by the same language, as if to attest the unity and simplicity of the plan of the universe, and to make still more evident that unchangeable order which presides over all natural causes."

We are now in possession of all the essential elements that go to make up the electro-magnetic telegraph: the voltaic battery, the magnetic action of the current, the laws of that action, and finally the law of Ohm. A new interest in all the variety of telegraphic and signalling appliances displayed at our Exhibition, dawns upon us when thus regarded as the direct and necessary product of researches such as these in the field of pure science. Yet a tithe of the important researches bearing on a line of practice such as that of telegraphy, could not be instanced. Enough has been cited to conclusively show, to what great element in

human progress, an exhibition of these practical results is chiefly debtor. But if it is still doubted whether these scientists ever know what they are doing, and what for, they are doing it, we must remind you how it was in the very highest realms of pure research that the first suggestion of the magnetic telegraph was made, and also the first actual telegraph line constructed and used. For it was within three months after the knowledge of Oersted's experiment, that Ampère had actually proposed the first plan for a telegraphic communication by means of electro-magnetism. And it was between the Astronomical Observatory and the Physical Laboratory at Göttingen that the eminent scientists, Gauss and Weber operated the first practical telegraph line.

The dynamo-electric machines displayed in such variety, and destined to several uses of profoundest interest to us all, will ever remain the prominent features of the Exhibition; as in memory, we pass through the fascinating scene. Those dynamos always excited our respect. Their very crudeness did not even detract from this feeling. There was about them a certain masculinity of character that impressed itself on everyone, and gave a tone of work-a-day earnestness to the entire Exhibition. This earnestness of spirit was real and gave promise of a vigor of progress for the future, that should equal, and doubtless excel, the historical development of the steam-engine.

While the variety of type pleased the mechanical fancy, it did not daze the scientific analysis. However crude or however typical of what the future dynamo may be expected to be, each machine was readily analyzed into its essential elements and the action of these referred to the original principles of science.

There are two great events in science that made the dynamo-electrical machine possible; one is that of the discovery of magneto-electric induction by Faraday, in 1831; the other that of the establishment of the great laws of the correlation of forces and the conservation of energy, during the fourth and fifth decades of this century by a goodly array of mathematical and experimental genius. Without the well-known researches of Faraday on magneto-electric induction and the consequent development of the magneto-electric machine from the early Dal Negro and Pixii types up to the Alliance and Pacinnotti, there could not have been a suitable preparation for the improvements of a Siemens or a

Gramme. And as an ever-present reminder of the value and eventual use to mankind of such researches, there should be stamped upon every dynamo the name of Michael Faraday.

Again, it is quite remarkable that the title of Werner Siemens' article, in which he in 1861 announced the dynamo-electrical or reaction principle, should have read: "Concerning the Conversion of Mechanical Energy into Electrical Current, without the Use of Permanent Magnets." It is but an evidence that the other great discovery in science referred to, had borne one of its legitimate fruits. Everyone is now familiar with the history of the discovery, proof and application of the laws of energy by Mayer, Joule, Thomson, Helmholtz, Clausius, and others; and everyone also with the kind of science these names imply.

We can therefore pass on to another definite instance of the fruits of the law of conservation of energy that should be mentioned in this connection. We have already spoken of Ohm's law, which is, of course, also here applicable in the dynamo; but we purposely, hitherto, omitted to mention the second law of electrical currents, because of its peculiarly useful relations to problems concerning dynamo currents. According to this law heat is generated or work performed in any given circuit in the following relation: quantity of heat = square of current × resistance × time. There is scarcely a practical calculation concerning the performance of the dynamo into which this little formula does not enter. It is the key that is constantly in the hands of the practitioner and that easily unlocks the dynamical riddle of the new engine. This time it is the experimental researches of Joule and Lenz, which first proved the relation, and it is the mathematical skill of Clausius that finally expressed the law as a deduction from principles already known.

It would scarcely add to the force of our main argument if we should go further and show how the whole dynamo-electric-engine has been subjected to a series of scientific tests, and of theoretical discussions which are destined to make a special kind of engineering. Enough that we can now, at least with partial insight, derive the scientific origin of the dynamo from the old-time rubbing of a piece of amber.

We must also omit discussion of the deep obligation due by our Exhibition to pure research in developing the very steam-

engine necessary to the generation of the new form of energy. To mention the bearing of Regnault's researches and the influence of the mechanical theory of heat, would lead us far away from the electrical side, and is here unnecessary.

We will, however, as we glance at the work to which the current generated in the dynamo is put, remark that it was in Davy's laboratory that the electric arc was first brought out in all its glory. We now see *hundreds* of arc lamps above and around us. Turning to the soft and beautiful light of the incandescent lamps, we see in imagination the spiral of wire that, under the electric discharge, became glowing in Kinnersley's old air-thermometer. If we see the dynamo-current at work in the chemist's hands, we have again to recall Davy's experiments and Faraday's exposition of the laws of electrolysis. If we watch the electric-motors, as they drive all sorts of machinery, we are borne back in imagination to the year 1835, and with the great physicist, Jacobi, of St. Petersburg, we go sailing up and down the Neva in a boat propelled by a magneto-electric-engine, while we learn from him some of the laws underlying the wonderful result.

It is the ocean cable, only present in our Exhibition by proxy, that must next serve as the text for a brief reference to the beneficent influence of mathematical theory in general and of the theory of the potential in particular, in bringing about the great achievements. The practical electrician we have ofttimes met in our wanderings through the Exhibition, has spoken of the electrical "potential" as though that were something to put one's ignorance to the blush. But we have sometimes wondered whether he imagined that the word stood primarily in electricity for a purely mathematical conception, which he now unconsciously regards as a physical fact. Moreover, in the occasional references to the indebtedness of practical electricity to mathematical investigation, we have not been able to give even a partial idea of the theories developed, or of their bearing on practice. The only way in which we can pretend further to supply this omission, is to state, in a few sentences, the influence of a single mathematical conception on the progress of the computation of the electrical forces. Laplace, in treating the attractive forces of gravitation, had used a function essentially proportional to the attractive masses and the simple inverse distance. This function, V, has since become

famous for its power in dealing with all problems in which the forces act according to the inverse square of the distance. Green and Gauss first applied it to electrical problems, the latter to a general investigation comprehending gravitation, magnetism and electricity, and subsequently Sir Wm. Thomson and others used it as the very key to the solution of the most difficult problems. Now, to use the cable as an illustration of the practical bearing of researches apparently so far removed from actualities, we find that Sir Wm. Thomson, using the conception of the potential, already in 1855 derived the little formula which expresses the electrostatic capacity of a cable, considered as an immense leyden jar. We stand amazed when, after the breaking of a cable, we see a vessel sent to mid-ocean, there to put down grappling-irons through miles of water and successfully recover and splice the artery of commerce. But we should not forget the little formula, whose use could point to the very spot where the break must have occurred; and we should not fail to remember the formula's out-of-the-way origin.

As we take our parting glance of the fascinating and successful exhibition, we may sum up all in the statement that electricity is now subject to accurate measurement. The magnetometers, electrometers and galvanometers and the multiplicity of meters that measure every conditions of electrical action, stand as the crowning result of theoretical and experimental electricity. If we ask, why the instruments are of such and such forms, we are referred to the mathematics; if we inquire how they are used, we learn also the necessity of acquiring the art and practice of experimentation. If we inquire upon what general method the measurements are made, we are sent all the way back to the great astronomer Gauss, to learn of him the absolute system of expressing forces, and if we ask for the unit of measure, the yard-stick of the practical electrician, the so-called ohm, we have to intercede with the long line of eminent physicists, beginning with Wilhelm Weber and ending with Rayleigh and Rowland for a copy of their standards.

In every element of the Exhibition we detect the most undoubted relations to pure research; and when we run over the scene even thus cursorily, we behold pure research the presiding

genius, on whose behests depend the successful activity of every department of electrical practice here represented.

The lessons to be drawn from this review are evident. Research in America should be raised to a higher plane and such methods adopted to bring about the desired result as may have the sanction of past success. The reason why there are not more American names associated with the progress of physical research (and there are many more in this department than we have had occasion to mention), does not lie in the lack of interest, or the lack of power in the American student. Apart from the materialistic spirit that we all imbibe, there is the lack of the appliances and particularly of the unrestricted devotion that such work demands.

Speaking of appliances it is at least in the separation from first-class scientific libraries that the American student suffers untold inconvenience. European students in physics would be amazed to learn that in an American city of nearly one million of inhabitants, there could not be found a complete set of, say, Poggendorf's Annalen or Crelle's Journal, or an instance of other volumes which it were occasionally desirable to refer to, in order that experimentation might start in at the right point and lead to the right end. Especially, however, do we wish to insist on the fact that sufficient opportunity is not afforded for that jealous, single-minded attention which science requires. It is not that large endowments are not given to erect fine buildings for laboratories or to provide fine instruments. But it is that there are but few foundations which put the man back of the instruments and allow him to devote himself with undisturbed purpose to the work of his laboratory. If you will recall the influence of but one institution where a foundation is provided which places an unhampered man in the laboratory, you will appreciate the justice of these remarks. The Royal Institution of London was founded, we are told, to promote, among other things, scientific research. To do this it founded a professorship, which gave the incumbent free scope to follow out researches of the purest type. You know what has been done for electrical science by its professors, Davy and Faraday. They discovered the decomposition of the fixed alkalies, the laws of electro-chemical decomposition, the fact of magneto-electric induction and the two-fold magnetism of matter, together

with a whole body of general information on electrical subjects. You know then how deep a debtor our Electrical Exhibition was to this single foundation for the promotion of research. It need only be said that the prospect is clearly open of doing not only important service to science, but some day to humanity at large, by diverting some of the American wealth to such noble foundations. May we not hope that our public-spirited citizens will thus endow our own INSTITUTE, an institution in many respects similar to the one just mentioned. Such a happy result would certainly show that America was not altogether oblivious of its lasting obligation to the contributions already made toward its material and social progress, by the eminent cultivators of pure research.

ON THE FRICTION OF NON-CONDENSING ENGINES.

BY R. H. THURSTON, ITHACA, N. Y.

[*A Paper read at the Meeting of the American Society of Mechanical Engineers, New York, November 30, 1886.*]

The assumption of the distinguished engineer, De Pambour, that the wasteful resistance of a steam-engine consists of a constant quantity, the friction of the unloaded engine, increased by some increasing function of the added load, has been accepted as correct by probably all recognized authorities since his time. Calling R_0 the resistance of the engine running free and under no other load than its own friction, and calling R_1 the resistance coming upon it as a useful factor of its work, and making f the coefficient measuring the proportion of increased friction due to the load, the total resistance to be overcome by the engine piston is thus

$$R = (1 + f) R_1 + R_0 \qquad (1)$$

So far as the writer has observed it has never been questioned whether the quantity f is constant or variable, and no recent attempts have been made to ascertain its value by experiment.

It has long been the intention of the writer to settle this question, which had for years existed in his own mind, and the opportunity has recently been offered to do so, at least as that question affects the modern forms of non-condensing high-speed engines now so generally in use, especially for electric lighting purposes.

The first investigation was made at the suggestion of the writer and under his general direction, in the winter of 1883–84, upon a "straight-line engine," exhibited that year, at the Annual Exhibition of the American Institute, by the Straight-Line Engine Company, of Syracuse, N. Y., and built by them from the designs of Professor John E. Sweet, the inventor of its special features. The work was done with equal care and skill by Messrs. Mitchell and Aldrich, graduates of Stevens Institute of Technology, of the class of 1884. The results were sufficiently exact and satisfactory in every respect to have been made the basis of the conclusions here to be

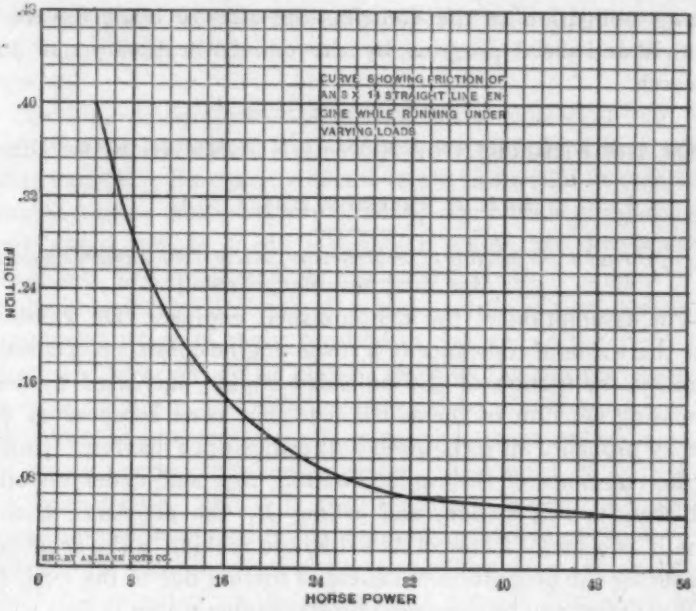

FIG. 4.

stated; but it seemed to the writer desirable that they should be checked by similar work upon another engine, if possible of a different make, before attempting to state definite conclusions of any kind. The opportunity to secure such a repetition of the investigation was offered, during the past winter, at Cornell University, using a straight-line engine, which could be fitted with a brake, and conveniently submitted to test. The engine is of the same make as the first described, but of a different size, and the results of the two sets of experiments are considered to accord so thoroughly as

to justify publication. The following are the data and results of these two sets of determinations:

The first of these two engines was built from designs brought out in the year 1880, of which illustrations may be seen in the *Electrician* of December, 1883. As is well known, the engine derives its name from the fact that, in its design, the attempt has been made to take all stresses through straight members, the frame thus being made to consist of two straight compression and thrust members, connecting the cylinder-heads directly with the main pillow-blocks, and giving a characteristic appearance to

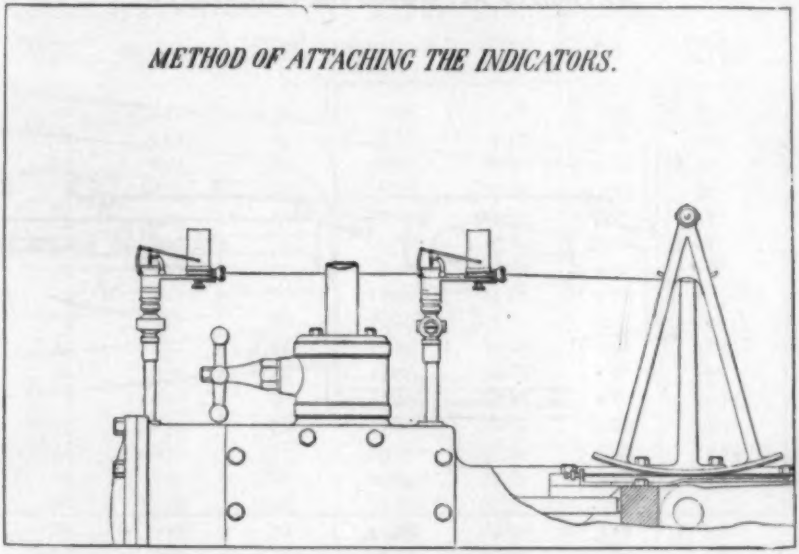

FIG. 1.

the whole machine. The valve-gear is of the "positive" type, the expansion made variable by the introduction of a governor on the main-shaft actuating the eccentric, in the manner familiar to all who have seen the more common forms of high-speed engines. In the design of this governor, as throughout the whole engine, special care has been taken to provide against the impeding action of friction, the machine being intended to be as nearly frictionless as possible. The engine rests upon three points of support, and thus is not liable to be thrown out of line by any inequalities of foundation or bolting. When tested, the engine to

be experimented with was simply set on blocking, and had no foundation; but so well was it balanced, and so perfectly was its alignment maintained, that it ran with absolute smoothness, and as steadily as if it had been given the heaviest foundation possible.

For the purposes of test, it was fitted with a pair of carefully standardized indicators and a Prony brake. Cards were taken simultaneously from both ends of the cylinder, and readings from the brake were at the same instant obtained. A comparison of the power indicated by the diagrams, with that shown by the brake

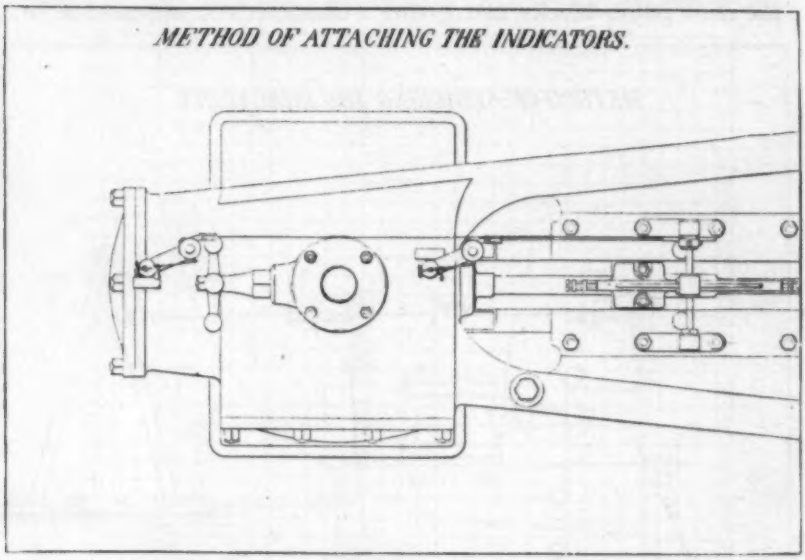

FIG. 2.

gave a difference which measured the friction of the engine. During the trial, the engine, when working at its rated power, consumed, according to the indications of the diagrams, 28·2 pounds of steam per horse-power per hour, or probably between thirty-five and thirty-eight pounds, allowing for the loss by cylinder condensation not accounted for on the indicator card, a very excellent performance for an engine of but thirty-five horse-power. The action of the governor was extraordinarily perfect. The engine was adjusted to make 230 revolutions per minute under ninety pounds steam-pressure. The observers reported that it made the same number of turns whether loaded or unloaded, an evident

impossibility with a governor of this class, in which only approximate isochronism can be attained. The writer, to settle the question, counted the revolutions, minute by minute, with a hand-speed counter, and made it 230 revolutions with the whole rated load on the engine (thirty-five to forty horse-power), and 231 when entirely unloaded, the brake-strap being loosened until it could be shaken about on the pulley, by the hand, with perfect ease. This was repeated until no question could longer exist in regard to the matter. The variation with variable steam-pressure was greater.

The following are the data obtained from the brake and indicator readings:

Number of Card.	Revolutions.	Steam-Pressure.	Brake H.-P.	Indicator H.-P.	Diff.	Friction per Cent.
1	232	50	4·06	7·41	3·35	45·
2	229	65	4·98	7·58	2·60	34·
3	230	63	6·00	10·00	4·00	40·
4	230	69	7·00	10·27	3·29	32·
5	230	73	8·10	11·75	3·65	32·
6	230	77	9·00	12·70	3·70	29·
7	230	75	10·00	14·02	4·02	28·
8	230	80	11·00	14·78	5·78	25·5
9	230	80	12·00	15·17	3·17	21·
10	230	85	13·00	15·96	2·96	18·5
11	230	75	14·00	16·86	2·86	17·
12	230	70	15·00	17·80	2·80	15·75
13	231	72	20·10	22·07	2·06	9·
14	230	75	25·00	28·31	3·36	11·75
15	229	60	29·55	33·04	3·16	9·5
16	229	58	34·86	37·20	2·34	6·3
17	229	70	39·85	43·04	3·19	7·4
18	230	85	45·00	47·79	2·75	5·8
19	230	90	50·00	52·60	2·60	4·9
20	230	85	55·00	57·54	2·54	4·4

This engine was 8 inches in diameter of cylinder, 14 inches stroke of piston, having a rod 44 inches long between centres, a balanced-valve with stroke of 2 to 4 inches, according to position of governor and eccentric, a fly-wheel 50 inches in diameter, weighing 2,300 pounds, the stem and exhaust-pipes having diameters of 2½ and 4 inches, respectively, and the whole machine weighing two and one-half tons. The space occupied by the engine was 9 feet 4 inches in length, by 4 feet 8 inches in width, and 3 feet 10 inches in height.

Examining the above table of powers, it is seen that the difference between indicated and dynamometric power, *i. e.*, the friction of the engine, varies somewhat with varying steam-pressures and varying total power; but in such manner as to indicate the controlling cause to be irregular in action, and possibly to some extent due to errors of observation and to accident. The maximum

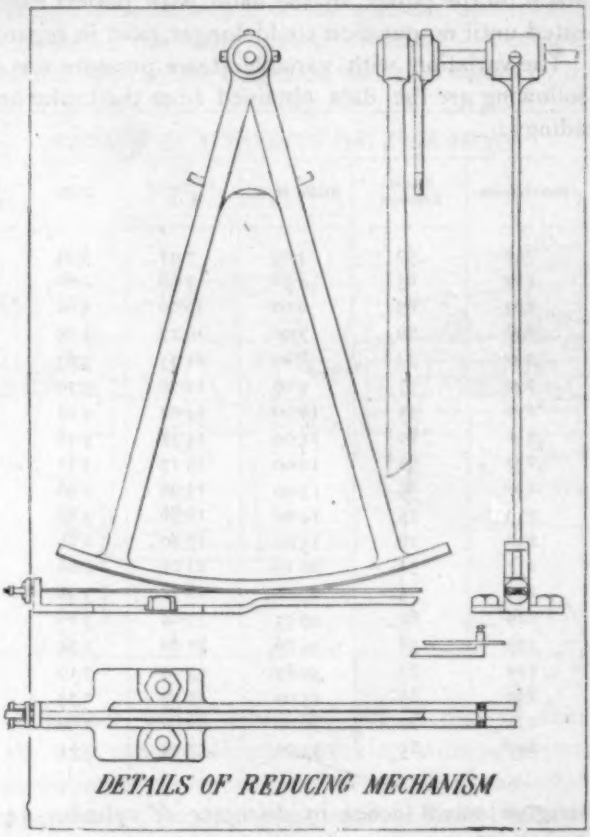

FIG. 3.

is four horse-power, the minimum about two horse-power. The usual difference is about three and the variations are irregularly distributed throughout the whole range of experiments. It is evident at a glance that the law of De Pambour does not hold, and that it is much nearer correct to say that the friction of engine is constant as otherwise. The column of friction, as given in per-

centages of the total power, exhibits the same fact. There is continual, though somewhat irregular, reduction of the percentage of friction, throughout the range from the lowest to the highest power, and very nearly inversely as the power exerted. This is best shown by the curve given in the accompanying plate, *Fig. 4*, in which a smooth line has been drawn to represent as nearly as possible the mean of all observations. It is evidently more nearly correct to assert that the friction of a non-condensing engine of this class is constant, and independent of the total power developed, than to accept the rule of De Pambour. The power for which the engine is proportioned is thirty-five to forty horse-power. At this power, the friction of engine is but about six per cent. of the total, or less than one-half that assumed by De Pambour, and accepted as correct by Rankine, for engines generally, and presumably for locomotives especially. The result is exceedingly gratifying, and the friction seems to the writer extraordinarily low for so small an engine.

The repetition of the experiment upon an engine of another make, having a cylinder 9 inches in diameter and a stroke of piston of 12 inches, which would naturally give a somewhat increased percentage of friction, in consequence of the proportionally smaller stroke, at twenty, thirty, fifty and sixty-five horse-power, by brake, and running free, revolutions 300 per minute—a speed which may also have caused some increase in frictional resistance, not only in rubbing parts, but by increasing back pressure—gave a friction of engine measuring from 2·66 horse-power unloaded, to four horse-power at twenty to thirty horse-power, 4·8 horse-power at fifty, and 5·3 at sixty-five horse-power, the total friction increasing perceptibly, as assumed by De Pambour, but decreasing in percentage of load, from 16 to 7·5, between twenty and sixty-five horse-power. It is very nearly constant throughout the whole range of power that the engine would be worked through under ordinary circumstances, and may be so taken without serious error; while the adoption of the Pambour formula would give a value of f so small that its use would not be attended, ordinarily, with sufficient increased exactness to compensate the additional trouble involved in its application. At their rated powers the two engines thus exhibit efficiencies of mechanism of about ninety-four and ninety per cent. respectively.

The second series of experiments were made by Messrs. W. A. Day and W. H. Riley, at Cornell University, during the latter part of last college year, confirming the deductions already given, while some very interesting and original modifications were made in the details of method and trial. The engine taken for test was a machine recently built, and sent to the Cornell University, for purposes of experimental investigation in electrical measurement and other work of the college. It is an engine 7 inches in diameter of cylinder and 12 inches stroke, or, more exactly, 6½ inches in diameter, the cylinder having been bored slightly under size. The general plan of the engine is similar to the first of those already described, and, like that, is carefully designed with a view to reducing friction to a minimum and giving a regulation of maximum efficiency. The brake was precisely like that used in the first described experiments, and was built for the engine constructed in the college work-shops, under the direction of the inventor, and exhibited at the Centennial Exhibition, in 1876. It was constructed by the Straight-Line Engine Company, and adapted, with very little alteration, to the new engine. The indicators were carefully standardized and put in good order in every respect, by the makers, for the purposes of these investigations. The reducing mechanism used in connecting the indicator-barrel to the cross-head of the engine was designed and built by the observers, and fitted with a very firm connecting arrangement, and with an ingenious detaching device (*Figs. 1, 2, 3*). A sector was constructed which was pivoted above the cross-head and hung in the vertical plane above the latter, the engine being horizontal. The arc of the sector carried a pair of steel ribbons, one attached to each end, each carried around the arc and secured, at its opposite end, to the end of a bar fastened on the cross-head, in such manner that, the two ends of the ribbons at the cross-head bar being well secured and tightly drawn up by means of screws placed conveniently for the purpose, all back-lash was prevented, and an absolutely exact synchronism of movement of indicator-line and cross-head was obtained. The engine was driven at 285 revolutions per minute, and it was, therefore, very important that this rigidity of connection should be secured. A smaller sector at the upper part of the larger one was the carrier of the cord, and the combination was thus a perfect means of reproducing the motion of the engine on

the smaller scale required in working the paper-barrel of the indicator. The "cord" was piano-wire, a material much less liable to cause difficulty by stretching than any other that was available. Its free part was kept taut by a "spiral" (helical) spring, attached beyond the point of connection with the paper-cylinder.

In the first of these experiments, as already described, Thompson indicators were used; in those about to be considered Crosby instruments. It was hoped that the new Tabor indicator could be used also, but none were received in time. The instruments

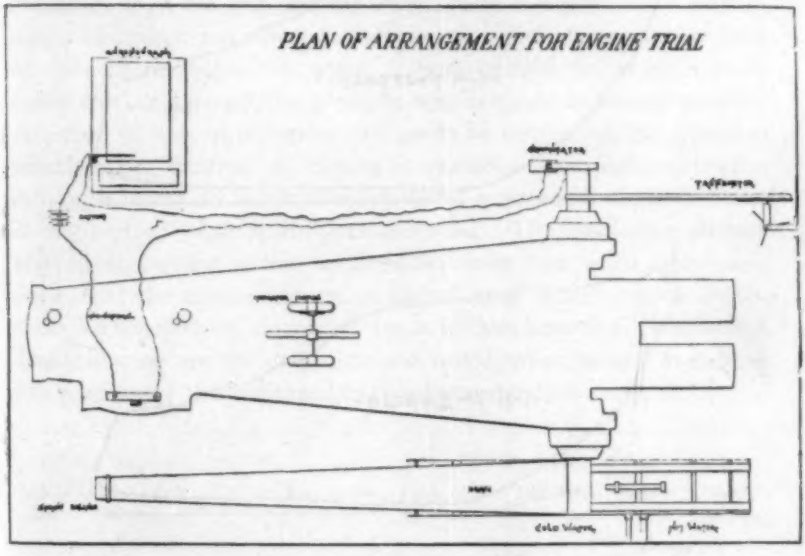

FIG. 5.

used worked perfectly, and gave no trouble from beginning to end. The speed indicators were of several kinds. Hand instruments of two or three kinds were used to check the records of the automatic instruments. A "tachometer" was attached and belted to the engine-shaft, and afforded a very convenient means of watching the momentary fluctuations due to variations of load, of steam-pressure, and to accidental disturbances. A chronograph was also attached, connected with the standard clock in the physical laboratory, to beat seconds. A commutator was placed on the engine-shaft, making contact at each revolution, and a key near the engine, for the purpose of breaking contact. A Brown mercury

speed-indicator served excellently well for a constant speed-indicator. It exhibited instantly any variation of speed from the normal. The chronograph was set in operation when the indicator-cards were taken, and thus gave the exact speed of the engine at that instant. Great care was taken to keep the instruments, and the

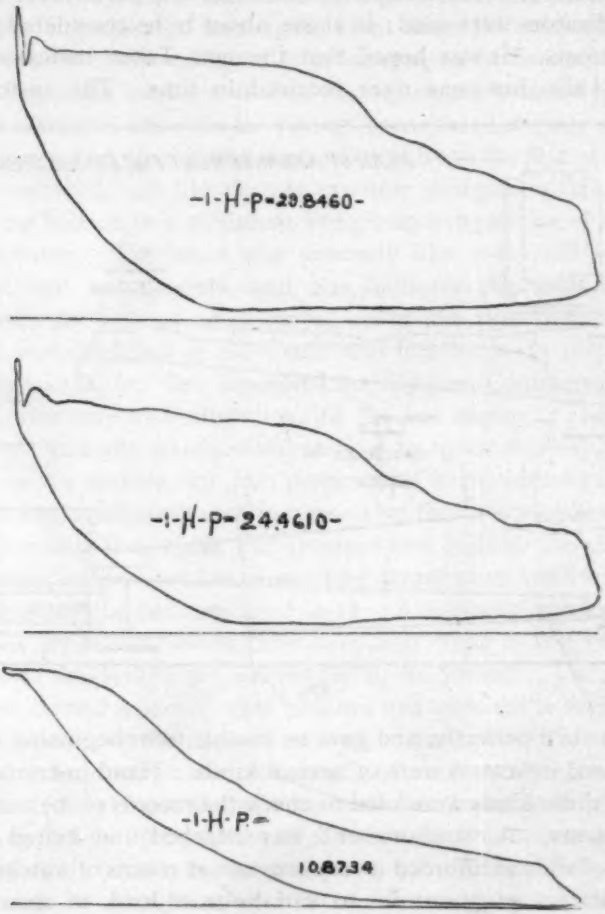

FIG. 6.

engine as well, in good order and well lubricated throughout the series of experiments. Some stiffness of the governor, however, the cause of which was not discovered until after the work had been completed, caused it to work less perfectly than in the engine first

used, and the speed varied more than in that series of determinations. When the governor was in its most perfect adjustment, the engine was capable of holding the standard speed within a fraction of one revolution throughout a wide range of work, and nearly down to the lowest power that such an engine is at all likely ever to be called upon to supply.

The mean effective pressure required to drive the engine itself, loaded and unloaded, throughout the whole range of the trials here made, was 4·55 pounds per inch of piston, and was nearly constant, as in the first investigation. The steam-pressure usually ranged between sixty-five and seventy-five pounds per square inch at the steam-chest; but, when it was desired to secure a more easily worked-up card, the pressure was dropped to twenty pounds. A series of special experiments made to determine the question whether the friction of engine is variable with boiler-pressure, although not in all respects satisfactory, indicated a slight increase in engine friction as steam-pressures rose. The conclusion already arrived at by the writer, as deduced from the work previously done, that the engine-friction, in this class of steam-engine is constant, or sensibly so, under all loads is thus here again confirmed. The following are the data obtained, arranged as before, to exhibit the relation of the indicated to the dynamometric powers:

1. No. of Card.	2. Rev. per Minute.	3. St.-Press.	4. Brake-Power. H.-P.	5. Ind. H.-P. per Card.	6. Diff. Frict. H.-P.	7. Mean F. Press.	8. Frict. per cent.
1	282	19	0	2·26	2·26	3·70	100
2	288	65	4·87	8·43	3·56	5·56	42
3	286	66	7·61	10·95	3·33	5·25	30
4	284	65	10·30	12·93	2·89	4·13	20
5	285	71	13·10	15·99	2·61	4·25	18
6	284	76	15·80	18·79	2·99	4·71	16
7	284	74	18·55	20·73	2·65	4·18	12
8	280	67	21·00	23·73	2·73	4·37	11
9	279	65	23·61	25·95	2·33	3·73	9
10	280	75	26·39	29 95	2·36	5·38	11
11	280	72	29·03	32·22	3·19	5·15	10

The first glance at column 6 or at column 7 of the above table, in which the horse-power absorbed by the friction of the engine,

and the mean effective pressure corresponding to that power are presented, shows that, as already concluded, the resistance of this class of engine at constant speed, is practically constant at all loads and that the differences and irregularities observed are due to accidental causes. The variation of speed recorded here is in some cases due to differences of steam-pressure, partly purposely produced, and partly coming of the fact that it was necessary to take steam as it could be obtained, and was impracticable to secure steady pressure, and in other instances was due to the fact, afterward discovered, that the governor had been adjusted in such manner as to be slightly cramped, and thus deprived of its wonderful sensitiveness and accuracy, as exhibited before this defect had been introduced, and after it had been remedied. Chronograph records, made later by Professor Anthony, exhibit the most extraordinary smoothness.

No. of Cards.	Rev.	St.-Press.	I. H.-P.	Mean Press.	Mean F. Press.	Per cent. Frict.	
1	250	25	6·01	10·84	1·95	18	
2	271	39	6·52	10·85	2·71	27	
3	285	42	7·17	11·35	3 63	32	
4	280	46	7·08	11·60	3·59	31	
5	271	58	6·81	11·28	3·16	28	
6	289	63	7·85	12·25	4·65	38	Ten pounds on the brake.
7	286	68	7·77	12·25	4·90	40	
8	283	77	7·88	12·47	3·74	38	
9	296	82	7·87	12·00	4·68	39	
10	275	71	2·10	3·46	3·46	100	
11	279	66½	1·995	3·22	3·22	100	
12	277	44	1·708	2·78	2·78	100	
13	275	35	1·71	2·80	2·80	100	
14	275	30	1·613	2·64	2·64	100	No load on the brakes.
15	272	25	1·876	3·11	3·11	100	
16	270	19	1·724	2·88	2·88	100	
17	270	15	1·712	2·86	2·86	100	

These variations of speed served the useful purpose of calling attention to the fact that the engine-friction varied, at constant load and speed, with variation of steam-pressure, and to a very noticeable amount, within the usual range of pressures met with in practice. It is seen that, in rising from nineteen to seventy-six

pounds steam-pressure, the pressure demanded to give the engine its normal speed unloaded ranged from below four to above five pounds per square inch of area of piston, the pressure required in the cylinder rising, on the whole, though irregularly, as steam-pressure rose. In order to determine whether this, which might prove to be a hitherto unobserved law, were true, the foregoing data were obtained by a series of experiments made for the purpose of settling this new question.

In the first set of experiments, here numbered 1 to 9, inclusive, the weight on the brake-arm was kept constant at ten pounds; in the remaining experiments, all weight was removed. In both sets, the same general effect is seen. As the steam-pressure rises, the speed being the same and the resistance the same, the friction of the engine increases; from two pounds, at twenty-five pounds pressure in the steam-chest, to nearly five pounds per square inch of piston at the maximum, eighty-two pounds steam in the valve-chest. As the steam-pressure fell from this point to fifteen pounds in experiments 9 to 17, the load being thrown off entirely and the speed being nearly constant, the mean pressure measuring the friction of engine falls again below three pounds per square inch of piston. The difference is considerably less in the last series than in the first, which apparent discrepancy is accounted for by the fact that the variation of steam-pressure in the first series was accompanied by a greater change of speed of engine than in the second. The resistance is seen to increase slowly, therefore, with increase in speed of rotation. The effect of change of pressure is, in these cases, more marked than that of alteration of velocity of the engine.

The accompanying plates illustrate the apparatus and exhibit the facts revealed by the investigations which have now been described better than can the text. *Fig. 1* shows the method of attaching the indicators, with an elevation of the engine-cylinder and section at the cross-head; *Fig. 2* exhibits the same arrangement in plan; *Fig. 3* gives an enlarged view of the reducing mechanism and attachment to the cross-head; *Fig. 5* is an outline plan of engine and surroundings, exhibiting the location of instruments; and *Figs. 6* and 7 represent characteristic diagrams obtained by means of the indicator, showing the variations of steam distribution with variations of load on the brake. All these

illustrations refer to the work of later date. *Figs. 8* and *9* are given to exhibit the method of variation of mean friction-pressures

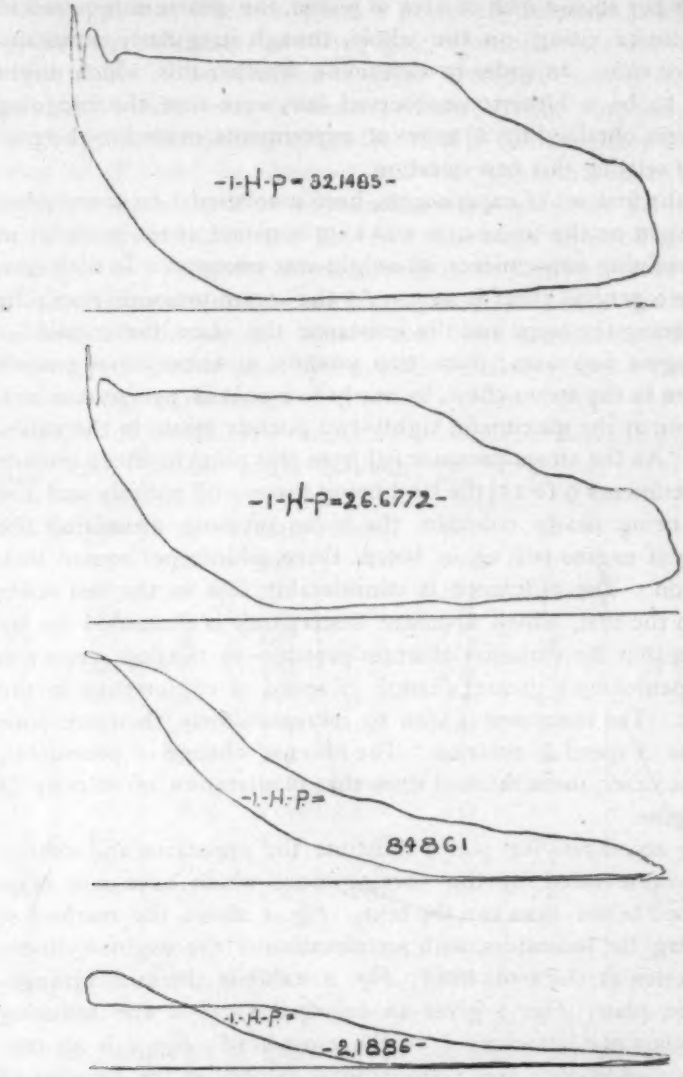

FIG. 7.

with variation of load, the variation of the percentage of friction resistance as a fraction of total resistance with varying loads, for

the last investigation; and, for comparison, the same ratios, as obtained in the work done at the American Institute Exhibition,

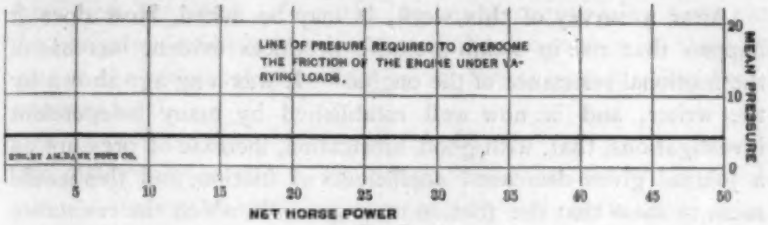

FIG. 8.

are given in *Fig. 4*. These last curves are seen to be approximately hyperbolic; while the first given is a straight line. The

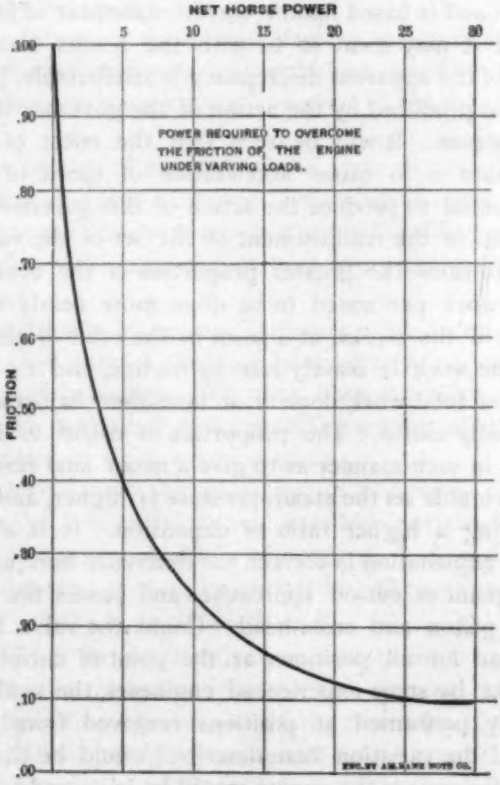

FIG. 9.

originals of these curves were carefully plotted by Messrs. Day and Riley, from the records of original observations, and beauti-

fully represent the law which it was the object of these investigations to reveal and establish.

After a survey of this work, it may be asked, How does it happen that rise in steam-pressure produces evident increase of the frictional resistance of the engine? It was long ago shown by the writer, and is now well established by many independent investigations, that, with good lubrication, increase of pressure on a journal gives decreased coefficients of friction, and this would seem to show that the friction of engines in which the resistance caused by friction is mainly due to journals and lubricated surfaces, should become less as pressures increase, the useful load and the speed of engine remaining constant. This query is a very natural one, and is based upon a correct statement of fact, however inconsistent it may seem to be with the results above derived. The cause of the apparent discrepancy is attributable, probably, to the variation produced by the action of the governor in the distribution of steam. It will be seen that the effect of increase of steam-pressure is to cause acceleration of speed of engines, a change essential to produce the action of the governor at all, and that it results in the readjustment of the set of the valve in such manner as to cause the greater proportion of the nearly constant amount of work performed to be done more nearly at the commencement of the stroke, at a point in the orbit of the crank-pin at which the work is mainly lost by friction, and to reduce the proportion of total work done at or near the "half-centre," where it is principally useful. The proportion of useful to lost work is thus varied in such manner as to give a mean final result which is the less favorable as the steam-pressure is higher, and the cut-off shorter, giving a higher ratio of expansion. It is also evident that, if this explanation is correct, the difference here noted will be less as the point of cut-off approaches and passes the half-stroke position of piston and cross-head. Could the valve be set with negative lead for all positions at the point of cut-off, as is considered right by some experienced engineers, the work would be more nearly performed at positions removed from the "dead points," and the variation here described would be thus reduced, while the efficiency of the engine would be increased.

Professor Rankine proposed the formula,

$$R = R_1 (1 - f) \qquad (2)$$

This formula is evidently inadmissable, at least for the class of engine which was made the subject of the experiments which have been here described. Since the friction of engine is, so far as can be here seen, sensibly independent of the magnitude of the load and of the resistance produced by it, the correct formula would seem to be

$$R = R_1 + R_0 \qquad (3)$$

the total resistance met at the piston being the sum of the resistance of the engine itself and that of the load, both being determinable, both being independent, and being governed by entirely different laws.

The conclusions to be drawn from what has preceded are obviously the following:

(1.) The friction of the non-condensing engine of the class here described, is sensibly constant at any given speed at all loads, and is at different speeds entirely independent of the magnitude of the load.

(2.) The friction of engines of the type described is variable with variation of speed of engine, increasing as speed increases, in some ratio as yet undetermined, but probably different with every engine, and, for the same engine, with every change of conditions of operation.

(3.) The friction of engines increases with increase of steam-pressure, in the case of the class here referred to, in a probably similarly variable manner with that observed with alteration of speed, neither method of variation being capable of representation by any convenient algebraic expression.

(4.) The total resistance measured at the piston of the engine is composed of two parts, the one sensibly constant at the working speed, the other variable with external load, and may be, for practical purposes, at least, represented by the expression,

$$R = R_1 - R_0,$$

in which R is the total resistance, as shown on the indicator diagram, R_1 the resistance due to the external load; *e. g.*, as measured by a Prony brake, and R_0 the resistance of the unloaded engine, as shown by a "friction-card" taken with the steam-engine indicator.

It is sufficiently obvious that these conclusions are, at present at least, only certainly applicable to one class of engine. It is

not improbable that the condensing-engine may be subject to quite different laws. It is to be hoped that this question may be settled by direct experiment at an early day. The custom has obtained, hitherto, of allowing a certain pressure per square inch of piston as the equivalent of the friction resistance of the engine in marine practice—this pressure being taken at from two and one-half pounds in the case of engines of moderate size, to one and one-half with the largest engines. It has never yet been ascertained whether, or to what extent, the friction of engine is augmented by the imposition of load. The assumed figure represents from five to ten per cent., usually, of the total indicated power of the engine. Isherwood has taken seven and one-half per cent. of the useful load as the amount of increase of friction of engine due to its action. This estimate is stated to be made on the basis of the data given by General Morin, whose coefficients for friction of lubricated surfaces are now known to be enormously larger than those customarily met with in practice in well lubricated journals of large size working under heavy pressures. In such cases, when the surfaces are in good order, the coefficient is known to fall to below one per cent., instead of being from three to five, as given by Morin, as determined under the different conditions of his experiments. Where the journals are not well lubricated, and especially when they are rough or cut by abrasion, friction may increase enormously and may pass far beyond the figures given by Morin even; but such exceptional conditions cannot be taken into account to establish laws for application in design, or in good practice. For all cases in which the friction varies, as in the examples here above illustrated, the "friction-card" sensibly represents the correct tare, whether the engine be loaded or unloaded.

A word in explanation of the fact here shown, that the increased load thrown upon the shaft, crank-pin and cross-head journals does not noticeably increase the friction of engine, will be considered not out of place here. The friction of engine consists of the resistances due to the motion of the various piston, valve and other rods through stuffing-boxes and in guides, the friction of the piston-rings on the cylinder-surface, the friction of eccentrics, and often other parts which are independent of the magnitude of the load thrown upon the engine by the useful resistance, in addition to the

friction of the journals transmitting the effort of the steam to the exterior resisting work, and of the cross-head guides and other parts indirectly affected by its variation. It thus happens that the resistance due to the friction of the latter may be, and often is, but a small proportion of the whole friction of engine. The total friction of engine, as has been seen, in engines of the class here studied and of the sizes described, amounts to about ten per cent. of the total power developed when fully loaded; but the coefficient of friction of any one journal, if well lubricated, has been found by the writer, by hundreds of experiments, under such pressures as are usual on the main journals of the steam-engine, to fall below one per cent., and the absorption of work and energy is thus a still lower proportion of the work of the steam in proportion as the speed of rubbing is less than that of the piston. The loss of power along the line of connection is thus exceedingly small. It should never exceed probably two per cent. of the work done, or between ten and twenty per cent. of the total friction. Again, the coefficient of friction, within the usual range of pressures on these journals and the guides, with good lubrication, increases rapidly as pressures fall, and decreases as greatly when the pressures increase with variation of engine-power and load, and this change often occurs so rapidly that the total frictional resistance, on these parts even, varies very slowly with variation of load, while the friction of the other portions of the engine, above mentioned, remains quite constant. The resultant effect is, as shown by the investigation here described, a practically constant friction of engine under all loads, the speed and steam-pressure being constant. Whether this is true of condensing engines is doubtful, and it would be an important extension of this research could similar investigations be made of the friction of other forms, and especially the marine steam-engine and pumping-engines.

NEW ALLOY OF ALUMINIUM.—Bourbouze recommends an alloy of aluminium, ten parts; tin, one part; sp. gr, 285, as more easily worked and soldered than aluminium. It is also somewhat whiter.—*Comptes Rendus, June 7, 1886.*

EXTENSIONS OF RAOULT'S LAW.—Raoult finds that his law—that the solution of any organic molecule, not an acid or alcohol, in 100 molecules of liquid lowers the freezing-point about 0°·62—extends to the solvents thymol and naphthalene, whose melting points are high, 48°·55 and 80°·10, as well as to water, benzene, etc.—*Comptes Rendus, June 7, 1886.*

TURBINES.

By J. Lester Woodbridge, M. E., Stevens Institute of Technology.
[With an Introduction by De Volson Wood, Professor of Mechanical Engineering.]

(*Concluded from November issue.*)

We will now use these equations to show some errors made by Rankine and Weisbach. Rankine, in his *Steam Engine and Other Prime Movers*, discusses a special wheel of the Fourneyron type, in which he assumes

$$a_2 = a_1, \text{ and } \gamma_1 = 90°.$$

These in equation (15) give

$$v_1 = \sin \gamma_2 \sqrt{\omega^2 \rho_2^2 - 2\omega^2 \rho_1^2 + 2gH} \qquad (25)$$

This velocity is radial along the vane, and is called by Rankine the "velocity of flow." The tangential component of the actual velocity must in this case be $\omega \rho_1$, and this, by Rankine, is called the velocity of whirl (v), and his value of v on page 196, equation (9) of the *Steam Engine* is not only incorrect but meaningless even for the turbine he considers.

From equation (15a) we also have

$$v_2 = \sqrt{\omega^2 \rho_2^2 - \omega^2 \rho_1^2 + 2gH}. \qquad (26)$$

The expression for the efficiency becomes

$$E = \frac{\omega}{gH}\left[\omega \rho_1^2 - \omega \rho_2^2 + \rho_2 \cos \gamma_2 \sqrt{\omega^2 \rho_2^2 - 2\omega^2 \rho_1^2 + 2gH}\right]. \qquad (27)$$

This expression differs entirely from Rankine's equation (10), page 196. But running this wheel at the same speed that Rankine does his, Art. 175; that is, making the final velocity of whirl zero, we have

$$v_2 \cos \gamma_2 = \omega \rho_2,$$

and equation (26) becomes

$$\omega \rho_2 = \cos \gamma_2 \sqrt{\omega^2 \rho_2^2 - 2\omega^2 \rho_1^2 + 2gH},$$

from which we find

$$\omega = \sqrt{\frac{2gH}{2\rho_1^2 + \rho_2^2 \tan^2 \gamma_2}}; \qquad (28)$$

$$\therefore \omega \rho_1 = \sqrt{\frac{2 g H}{2 + \frac{\rho_2^2}{\rho_1^2} \tan^2 \gamma_2}}.$$

which is the same as Rankine's equation (3), Art. 175. Substituting these in (27) we have

$$E = \frac{2 \rho_1^2}{2 \rho_2^2 + \rho_2^2 \tan^2 \gamma_2}$$

which is Rankine's equation (4), Art. 175.

We thus see that Rankine's equations not only do not fit any wheel except the one he is considering, but they apply to that only at one particular speed. These conclusions agree with those in an article on "Turbines," by Professor Wood, in the JOURNAL OF THE FRANKLIN INSTITUTE, for June, 1884.

In regard to the speed for maximum efficiency, Rankine, in the *Steam Engine*, Art. 173, says: "In order that the water may work to the best advantage, it should leave the wheel without whirling motion; for which purpose the velocity of whirl relative to the wheel should be equal and contrary to that of the second circumference of the wheel." As plausible as this appears, it is true only for special cases even for his wheel. Also, Weisbach makes the erroneous statement that the velocity of the second rim of the wheel should equal the relative velocity of discharge. Thus, in *The Mechanics of Engineering and Machinery*, Vol II., page 400, of the translation by Du Bois, he says (substituting my notation for his):

$$w = \sqrt{\omega^2 \rho_2^2 + v_2^2 - 2 v_2 \omega \rho_2 \cos \gamma_2}$$

$$= \sqrt{(\omega \rho_2 - v_2)^2 + 4 \omega \rho_2 v_2 \sin^2 \frac{\gamma_2}{2}}$$

in regard to which he states that for w a minimum $\omega \rho_2$ must equal v_2, which is not *generally* true, and is true only when $\gamma_2 = 0$, or when $v_2 \omega \rho_2$ and $(\omega \rho_2 - v_2)^2$ happen to be a minimum together. The value of ω in equation (24) will in general satisfy neither Rankine's condition,

$$\omega \rho_2 = v_2 \cos \gamma_2, \tag{29}$$

nor Weisbach's,

$$\omega \rho_2 = v_2. \tag{29a}$$

Substituting in equation (24) Rankine's condition, $\gamma_1 = 90°$, and making $r = \frac{\rho_1}{\rho_2}$, we find

$$\omega^2 \rho_2^2 = \frac{g\,H}{1 - 2r^2}\left[\frac{1 - r^2 - \sqrt{(1 - r^2)^2 - \cos^2\gamma_2\,(1 - 2\,r^2)}}{\sqrt{(1 - r^2)^2 - \cos^2\gamma_2\,(1 - 2\,r^2)}}\right] \quad (30)$$

and

$$v_2^2 = g\,H\,\frac{1 - r^2 + \sqrt{(1 - r^2)^2 - \cos^2\gamma_2\,(1 - 2\,r^2)}}{\sqrt{(1 - r^2)^2 - \cos^2\gamma_2\,(1 - 2\,r^2)}} \quad (30a)$$

Substituting this in (15*a*) gives

$$v_2 \cos\gamma_2 = \omega\,\rho_2\left[1 - r^2 + \sqrt{(1 - r^2)^2 - \cos^2\gamma_2\,(1 - 2\,r^2)}\right]$$

This satisfies equation (29) only when $r^2 = \frac{1}{2}$, and (29*a*) only when $\gamma_2 = 0°$ or $r = 1$. The latter condition is that of a parallel flow wheel, or of an infinitely narrow wheel, in which cases we have

$$\omega\,\rho_2 = v_2 = \sqrt{g\,H}.$$

These in (17) give for the efficiency

$$E = \cos\gamma_2$$

which always exceeds the value given by Rankine,

$$E = \frac{2\cos^2\gamma_2}{2 + \cos^2\gamma_2},$$

except when $\gamma_2 = 0$, when both become unity, but the work done will be zero.

Making $\gamma_2 = 0$ in (24) and (15*a*), gives

$$\omega = \frac{\sqrt{g\,H}}{\rho_1}, \text{ and } v_2 = \frac{\rho_2}{\rho_1}\sqrt{g\,H};$$

$$\therefore\ v_2 = \omega\,\rho_2$$

for maximum efficiency, for this case, and the efficiency becomes unity.

In the case of Barker's mill, and the Whitelaw and other similar reaction wheels, we have $r = 0$, practically, and $\gamma_1 = 90$, and the angular velocity for maximum efficiency, equation (24), becomes

$$\omega\,\rho_2 = \sqrt{\frac{1 - \sin\gamma_2}{\sin\gamma_2}\,.\,g\,H}$$

and the final velocity, (15*a*),

$$v_2 = \sqrt{\frac{1 + \sin \gamma_2}{\sin \gamma_2} g H}$$

When $\gamma_2 = 0$, as is usually the case, $\omega \rho_2$ and v_2 become infinite according to the preceding equations, which results agree with common analysis.

In the general analysis when $r > 1$, we have the inward flow wheel. As r becomes greater, $\omega \rho_2$ and v_2 continually approach 0 in value, and their ratio $\frac{v_2}{\omega \rho_2}$ approaches the limit, $\cos \gamma_2$.

The value of the ratio

$$\frac{v_2 \cos \gamma_2}{\omega \rho_2}$$

indicates the direction of the final actual velocity. If that ratio be greater than unity, θ, *Fig.* 9, is obtuse. If less, θ is acute.

Table I exhibits these various quantities at the critical points, assuming $\gamma_1 = 90°$.

TABLE I. ($\gamma_1 = 90°$.)

r	$\omega \rho_2$	v_2	$\frac{v_2 \cos \gamma_2}{\omega \rho_2}$	E
0	$\sqrt{g H \frac{1 - \sin \gamma_2}{\sin \gamma_2}}$	$\sqrt{g H \frac{1 + \sin \gamma_2}{\sin \gamma_2}}$	$1 + \sin \gamma_2$	$1 - \sin \gamma_2$
$\sqrt{\frac{1}{2}}$	$\sqrt{g H \cdot 2 \cos^2 \gamma_2}$	$\sqrt{2 g H}$	1	$\cos^2 \gamma_2$
1	$\sqrt{g H}$	$\sqrt{g H}$	$\cos \gamma_2$	$\cos \gamma_2$
∞	0	0	$\cos^2 \gamma_2$	1

Table II gives the values of $\frac{v_2 \cos \gamma_2}{\omega \rho_2}$ and $\omega \rho_2$ for various values of r, assuming $\gamma_2 = 10°$, also the values of $\omega \rho_2$ deduced from Rankine's and Weisbach's conditions of maximum efficiency. Table III gives the same quantities when $\gamma_2 = 30°$.

TABLE II. ($\gamma_2 = 10°$.)

r	$\frac{v_2 \cos \gamma_2}{\omega \rho_2}$	$\omega \rho_2$ (Woodbridge.)	$\omega \rho_2$ (Rankine.)	$\omega \rho_2$ (Weisbach.)
0	1·174	$2·181 \times \sqrt{g H}$	$8·020 \times \sqrt{g H}$	∞
·2	1·131	$2·237 \times \sqrt{g H}$	$4·071 \times \sqrt{g H}$	$5·000 \times \sqrt{g H}$
·4	1·055	$2·070 \times \sqrt{g H}$	$2·387 \times \sqrt{g H}$	$2·500 \times \sqrt{g H}$
·6	1·012	$1·606 \times \sqrt{g H}$	$1·632 \times \sqrt{g H}$	$1·667 \times \sqrt{g H}$
·7	1·000	$1·393 \times \sqrt{g H}$	$1·393 \times \sqrt{g H}$	$1·414 \times \sqrt{g H}$
·8	·993	$1·242 \times \sqrt{g H}$	$1·235 \times \sqrt{g H}$	$1·250 \times \sqrt{g H}$
1·0	·985	$1·000 \times \sqrt{g H}$	$·992 \times \sqrt{g H}$	$1·000 \times \sqrt{g H}$
1·2	·980	$·835 \times \sqrt{g H}$	$·829 \times \sqrt{g H}$	$·833 \times \sqrt{g H}$
1·5	·976	$·668 \times \sqrt{g H}$	$·664 \times \sqrt{g H}$	$·667 \times \sqrt{g H}$
2·0	·974	$·500 \times \sqrt{g H}$	$·499 \times \sqrt{g H}$	$·500 \times \sqrt{g H}$
∞	·970	0	0	0

TABLE III. ($\gamma_2 = 30°$)

r	$\frac{v_2 \cos \gamma_2}{\omega \rho_2}$	$\omega \rho_2$ (Woodbridge.)	$\omega \rho_2$ (Rankine.)	$\omega \rho_2$ (Weisbach.)
0	1·500	$1·000 \times \sqrt{g H}$	$2·449 \times \sqrt{g H}$	∞
·2	1·441	$1·040 \times \sqrt{g H}$	$2·200 \times \sqrt{g H}$	$5·000 \times \sqrt{g H}$
·4	1·282	$1·150 \times \sqrt{g H}$	$1·750 \times \sqrt{g H}$	$2·500 \times \sqrt{g H}$
·6	1·087	$1·243 \times \sqrt{g H}$	$1·378 \times \sqrt{g H}$	$1·667 \times \sqrt{g H}$
·7	1·000	$1·225 \times \sqrt{g H}$	$1·225 \times \sqrt{g H}$	$1·414 \times \sqrt{g H}$
·8	·943	$1·168 \times \sqrt{g H}$	$1·113 \times \sqrt{g H}$	$1·250 \times \sqrt{g H}$
1·0	·866	$1·000 \times \sqrt{g H}$	$·926 \times \sqrt{g H}$	$1·000 \times \sqrt{g H}$
1·2	·826	$·846 \times \sqrt{g H}$	$·789 \times \sqrt{g H}$	$·833 \times \sqrt{g H}$
1·5	·796	$·678 \times \sqrt{g H}$	$·643 \times \sqrt{g H}$	$·667 \times \sqrt{g H}$
2·0	·775	$·506 \times \sqrt{g H}$	$·490 \times \sqrt{g H}$	$·500 \times \sqrt{g H}$
∞	·750	0	0	0

In *Fig. 10*, I have laid off the direction of the final actual velocity of the water for various values of r, taking the two cases $\gamma_2 = 10°$, and $\gamma_2 = 30°$, thus obtaining two curves, $O\,P'\,D\,K$, and $O\,P\,D\,L$. Taking any point P on one of these curves, $O\,P$ rep-

resents the value of r (OA being unity), and POA the corresponding angle θ (see *Fig. 9*). HE and GF are tangents at the origin, and OM and ON are asymptotes to the two curves, respectively. All curves such as these will pass through the

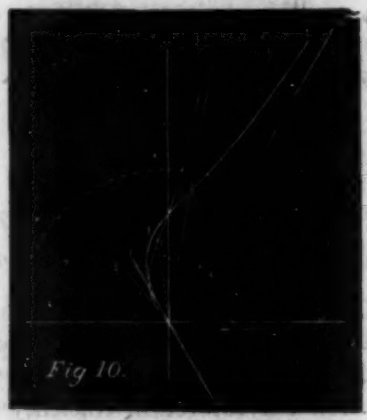

Fig 10.

point D, where $r = \sqrt{\frac{1}{2}}$, and the final velocity is radial for all values of γ_2 for this value of r. When $\gamma_2 = 0$, these curves reduce to the line OB.

In *Fig. 11*, I have plotted the values of $\omega \rho_2$, corresponding to

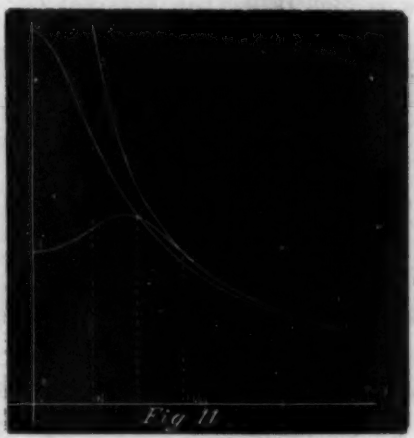

Fig 11

various values of r, for the condition $\gamma_2 = 30°$, thus obtaining the curve APB, and from Rankine's and Weisbach's conditions the curves GH and CD, respectively. The abscissas represent values of r, OM being unity, and the ordinates give $\omega \rho_2$, OA being

equal to $\sqrt{gH}$. Rankine's curve intersects the true one at Q, where $r = \sqrt{\frac{1}{2}}$, Weisbach's is an equilateral hyperbola, and is independent of γ_2. It intersects the true curve at S.

The above discussion applies only to the case where $\gamma_1 = 90°$. Returning now to the general case, substitute in equation (18) the value of ω^2 from equation (24) and we have for the maximum efficiency of any frictionless turbine

$$E_{max} = \frac{s^2 - \sqrt{s^4 - m^2 l^2}}{l^2} \tag{31}$$

This expression also involves the variables γ_1, γ_2, ρ_1, ρ_2, a_1, a_2. Putting $\frac{\rho_1}{\rho_2} = r$, and $\frac{a_2}{a_1} \cot \gamma_1 = f$, we reduce the number of variables to three. The complete discussion of this equation with reference to these three variables is too long and complicated for insertion here, and is not of much practical value. A few numerical results will show how the theoretical efficiency varies within the range of practice.

We observe first that the efficiency increases as γ_2 diminishes, and becomes unity when $\gamma_2 = 0$. But to insure a flow, γ_2 must have some value greater than 0. Letting $\gamma_2 = 30°$, we have the following tables.

TABLE A. (Values of the efficiency.)

f =	− ·2	− ·4	0	+ ·4
$r = \sqrt{\frac{1}{2}}$	1·000	·800	·750	·671
r = 1	1·000	·888	·866	·832
r = 2	1·000	·970	·968	·965

TABLE B. (Values of $\omega \rho_2$)

f =	− ·2	− ·4	0	+ ·4
$r = \sqrt{\frac{1}{2}}$	0	$1·099\sqrt{gH}$	$1·225\sqrt{gH}$	$1·321\sqrt{gH}$
r = 1	0	$·913\sqrt{gH}$	$\sqrt{gH}$	$1·118\sqrt{gH}$
r = 2	0	$·484\sqrt{gH}$	$·506\sqrt{gH}$	$·532\sqrt{gH}$

TABLE C. (Values of v_2.)

$f=$	$-\infty$	$-\cdot4$	0	$+\cdot4$
$r=\sqrt{\frac{1}{2}}$	0	$1\cdot267\sqrt{g\,H}$	$1\cdot414\sqrt{g\,H}$	$1\cdot623\sqrt{g\,H}$
$r=1$	0	$\cdot913\sqrt{g\,H}$	$\sqrt{g\,H}$	$1\cdot118\sqrt{g\,H}$
$r=2$	0	$\cdot438\sqrt{g\,H}$	$\cdot453\sqrt{g\,H}$	$\cdot479\sqrt{g\,H}$

TABLE D. (Values of θ. *Fig. 9.*)

$f=$	$-\cdot4$	0	$+\cdot4$
$r=\sqrt{\frac{1}{2}}$	90° 10′	90° 00′	84° 05′
$r=1$	75° 00′	75° 00′	75° 00′
$r=2$	64° 32′	63° 17′	64° 06′

The value $r=\sqrt{\frac{1}{2}}$ corresponds to an outward-flow wheel, $r=1$ to a parallel-flow, and $r=2$ to an inward-flow. The negative values of f occur when γ_1 is obtuse. In Table A, we see that the efficiency of the inward-flow wheel is greater than that of the outward-flow, other things being equal; and the efficiency is also increased by increasing the initial angle γ_1. When the latter becomes 180°, the efficiency becomes unity, but there is no flow. From

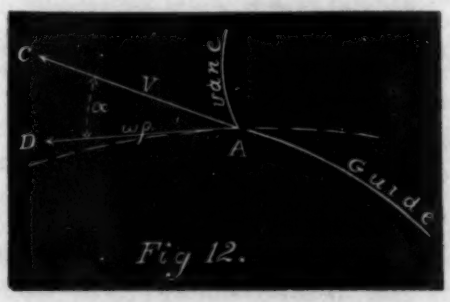

Fig 12.

Tables B and C, we see that for the parallel-flow wheel, $v_2=\omega\,\rho_2$ for all values of f, and the final actual direction of flow therefore bisects the external angle of the vane. In only one case is this flow radial. In the others, it has a tangential component, which is forward in nearly all the instances given above. It will be remembered that these tables apply to the wheel only when running at the speed of maximum efficiency.

In *Fig. 12*, let $A\,D$ be tangent to the wheel at its circumference,

$A\,C$ tangent to the guide-plate at its final element, and the angle between these tangents a, called the final angle of the guide-plates. Assume, as before, that this angle is so adjusted that the water will enter the wheel smoothly, then we have

$$V \cos a = \omega \rho_1 - v_1 \cos \gamma_1 \tag{32}$$

Combining this with (10), we have

$$\cos a = \frac{\omega \rho_1 - v_1 \cos \gamma_1}{\sqrt{\omega^2 \rho_1^2 - 2\,\omega \rho_1 v_1 \cos \gamma_1 + v_1^2}} \tag{33}$$

From this, v_1 could be eliminated by means of (15), and a expressed in terms of a_1, a_2, ρ_1, ρ_2, γ_1, γ_2, and ω. The first six of these quantities are fixed for any one turbine, while ω may vary. We thus see that our assumption in regard to a is equivalent to making that angle vary with every change in the angular velocity. This actually takes place to a certain extent by the piling of the water in the passages between the guides. Each turbine, however, is designed to run at a fixed speed, and, in order that this speed may be that of maximum efficiency, its dimensions and angles should be such as to satisfy equations (24), and the angle a should be determined from equation (33).

In order to discuss the pressure in the wheel, integrate equation (3) between the constant limits v_1, p_1, ρ_1, and the variable limits v, p, ρ, and find

$$\frac{1}{2}\omega^2(\rho_1^2 - \rho^2) - \frac{1}{\delta}(p_1 - p) = \frac{1}{2}(v_1^2 - v^2) \tag{34}$$

From (12) we have

$$\frac{p_1}{\delta} = \frac{p_0}{\delta} + g\mathfrak{h} - \frac{1}{2}v_1^2 - \frac{1}{2}\omega^2\rho_1^2 + v_1\,\omega\,\rho_1 \cos\gamma_1 \tag{35}$$

Also from (9)

$$v = v_1 \frac{a_1 \sin \gamma_1}{a \sin \gamma},$$

and these in (34) give

$$\frac{p}{\delta} = \frac{1}{2}\omega^2\rho^2 - \frac{1}{2}\omega^2\rho_1^2 + \frac{p_0}{\delta} + 2g\mathfrak{h} + v_1\,\omega\rho_1\cos\gamma_1 - v_1^2\frac{a_1^2 \sin^2\gamma_1}{a^2 \sin^2\gamma} \tag{36}$$

This gives the pressure at any point in the wheel in terms of constants already known, and the variables ρ, a, γ, depending upon the construction of the wheel. There are two distinct cases to be considered:

(1.) That in which the discharge is into the free air;

(2.) That in which the wheel is submerged.

In the first case, if equation (36) shows a gradually decreasing pressure from the entrance to the exit of the wheel, that equation holds good practically. If the pressure be constant, the case is that of free deviation, in which *the pressure does no work* directly in the wheel. If, however, equation (36) shows a point of minimum pressure, less than p_a, from which point the pressure increases both ways, the equation, while theoretically possible, indicates a condition of unstable equilibrium, which could never hold in practice. The slightest inequality in the action of the wheel would cause air to rush in, up to the point of minimum pressure, which pressure would then become that of the atmosphere. The pressures in the wheel, and consequently the flow and the entire action of the water would thus be changed. The wheel must now be considered in two parts —within and without the point of minimum pressure; and hence this point must be found before the wheel can be analyzed. If we know a and γ as functions of ρ, we could make $\frac{dp}{d\rho} = 0$ in (36) and determine this point analytically. But if, as is usually the case, we do not know the relations between γ, ρ and a, we must determine it graphically by plotting a curve of pressures from equation (36), substituting values of a, ρ, γ, taken at various points in the wheel; v_1 being found from equation (15), as if the passages were full. The point of minimum pressure may be found from this curve.

From the entrance of the wheel up to this point, the analysis will be the same as if the rest of the wheel were removed, since the water beyond is free, and exerts no reaction on the water behind. The equations already given, therefore, apply, and the velocity of the water at this point, which we will call v_x, may be determined from (15a). Beyond this, we have the case of free deviation; the pressure will be constant and equal to p_a, and equation (6) becomes

$$\omega^2 \rho_x^2 - \omega^2 \rho_2^2 = v_x^2 - v_2^2 \qquad (37)$$

This gives v_2 directly, and from this the work done in this part of the wheel may be calculated and added to that previously found.

If the point of minimum pressure is at the entrance to the wheel, the case becomes wholly one of free deviation.

The second case, that in which the wheel is submerged, we see that, since the air has no chance to enter, the equations will apply; subject, however, to the condition that the pressure, as given by (36), never becomes negative, since this would imply a tensile resistance in water. If equation (36) makes p negative, analysis becomes impossible, since the flowing stream would not fill the passages beyond the point of minimum pressure, and the effect of cushion water and eddying on the pressure and velocity, would enter as indeterminate elements. Such a wheel would never, knowingly, be constructed, and the analysis would be useless if possible.

PRACTICAL DEDUCTIONS FROM THE FRANKLIN INSTITUTE TESTS OF DYNAMOS.*

By Carl Hering.

The tests of dynamo-electric machines, made in 1885, under the auspices of the Franklin Institute, are undoubtedly the most reliable and complete of all the impartial tests which have ever been made and published, and they therefore afford the practical electrical engineer an excellent opportunity to deduce proportions, dimensions and constants, to assist him in designing dynamos, especially as the machines which were tested are among the best that are made, and represent the results of tedious and expensive experimenting on the part of the makers, while, at the same time, they embody the improvements suggested by long and continued use of the machines in practice.

Dynamos have frequently been built by "guessing" at the proportions, constructing them, and then trying them to "see what they will give." If they then turn out (by chance) to give the electro-motive force and current desired, the designer generally gets the credit for having made very correct "calculations;" while if they give, for some unknown reason, quite different results, the manufacturer has to be consoled with the statement that "it is not possible to calculate the parts of a dynamo."

If a machine is at hand which can be thoroughly tested and measured in all its parts, it is not difficult for a technical engineer,

* From the *Electrician and Electrical Engineer*, November, 1886.

	Reference Letter	Edison No. 4.	Edison No. 5 or T.	Edison No. 10 or S.	Edison No. 20 or H.	Weston 6 M.	Weston 6 W I.	Weston 7 M.
External diameter of armature in inches,	*a*	7·063	7.875	10·625	10·625	8·031	8·250	9·375
Internal diameter or diameter of the core,	*b*	6·250	7·063	9·688	9 688	7·387	7·410	8·595
Mean diameter in inches,	*c*	6·657	7·469	10·156	10·156	7·709	7·830	8·985
Speed in revolutions per minute,	*d*	1600·	1400·8	1208·6	1092·1	1114 9	1257·6	1043·5
Mean inductor velocity in feet per second,	*e*	46 5	45·6	53·6	48·2	37·4	43·0	41·0
Number of coils or commutator-bars,	*f*	50	50	64	44	72	56	64
Number of turns per coil,	*g*	2	2	1	1	2	2	2
Total number of turns on armature,	*h*	100	100	64	44	144	112	128
Mean distance between pole-piece projections in inches,	*i*	2 69	2·313	3·50	4·88	2·52	2·58	2·94
Active part of circumference of armature in per cent. (approx.),	*j*	76	80	80	71	80	80	80
Length of pole-piece or armature-core in inches,	*k*	12·	13·	16·5	29·5	14	14	16·5
Total E. M. F. in volts,	*l*	131·91	131·5	129·35	129·60	127·17	136·22	165·58
Mean induction in volts per foot,	*m*	1·733	1·515	1·838	1·687	·948	1·304	1·175
Volts per foot for one foot per second,	*n*	·0373	·0332	0343	·0350	·0254	·0303	·0287
Mean intensity of field, in lines of force per square inch,	*o*	25900·	23000·	23800·	24260·	17630·	21000	19900·
Amount of magnetism in field, in lines of force,	*p*	2660000	3090000	5250000·	8600000·	2490000·	3042000	3860000
Energy in field in horse-power,	*q*	·394	·401	·675	·862	·207	·402	·498
Useful lines of force per volt-ampère in field, (relative efficiency of field),	*r*	90·2	103 1	104·1	133·5	161·1	101·5	103·7
Length of wire in one coil in inches,	*s*	92·	102·	68·	98·	154·	151·	160·
Total length of armature wire in feet (parallel wires considered as one),	*t*	383	425	363	359	924	705	853
Available active length in feet,	*u*	152 0	173·4	140·8	153·6	268 8	209·0	281·6
Percentage of total, which is active,	*v*	40	41	39	43	29	30	33
Diameter of bare armature-wire in inches,	*w*	148	·180	·199	·199	·141	190	·175
Number of wires in parallel,	*x*	1	1	2	3	1	1	1
Total current in armature,	*y*	84·42	103·31	205·50	392·42	72·84	100·60	128·16
Square mils of cross-section per ampère,	*z*	407	492	606	475	428	562	374
Percentage of energy in armature,	*A*	3 70	4·65	3·12	3 39	5·43	3·09	5·59
Total electrical energy in horse-power,	*B*	14 93	18·23	35·67	63·25	12·43	18·39	28·47
Relative efficiency of armature as an inductor,	*C*	11·33	8·44	7·90	10 17	4·32	4·62	6·94
Useful commercial efficiency of machine,	*D*	88·38	89·76	89 70	91·90	87 38	90·21	89·92
Depth of winding in inches,	*E*	·407	·406	·469	·469	·322	·420	·390
Percentage of diameter taken up by windings,	*F*	11·5	10·3	8·83	8·83	8·02	10·2	8 32
Distance between pole-piece projections divided by depth of windings and clearance,	*G*	5 0	4·4	5·9	8·2	5·65	4·75	5·72
Ratio of length to diameter of armature-core,	*H*	1·9	1·8	1·7	3·0	1·9	1·9	1·9
Ratio of length to diameter of bearings,	*I*	4·2	4 3	4·1	4·1	4·2 & 3·6	4 2 & 3 6	3·8 & 4·3
Volts per commutator-bar,	*J*	6·7	6 25	4·9	7·2	4 2	5 9	6·1

who is well informed concerning the principles and practice of dynamo building, to calculate the parts of another machine of the same type which will give a certain desired electro-motive force and current slightly different from that of the first machine. But when the designer has no access to such a model machine, or if the current and potential desired differ greatly from those of the model, it is difficult, if not quite impossible, to calculate with any degree of certainty the parts of a dynamo from the principles and laws of induction and resistance, without some practical constants and proportions, which can be obtained only from existing machines.

As an aid and guide in such calculations, a set of practical constants and proportions have been calculated by the writer from the valuable tests of the FRANKLIN INSTITUTE. These constants will not only materially diminish the amount of "guessing" in designing machines, but it is believed they are sufficiently complete to enable a technical engineer to calculate all the electrical proportions of a well-designed cylindrical armature which is to give a certain required electro-motive force and current. It is to be regretted that the data given in the FRANKLIN INSTITUTE Report are not sufficient to enable a similar complete set of constants to be deduced for the field. A few of these may, however, be calculated, and they will materially aid in determining certain parts of the field. The calculations of armatures being based on induction and conductivity, the constants from one armature may be used in calculating others; but in the field this is not the case, as the relation between the exciting current and the magnetism produced depends very largely on the size, shape and proportions, of the coils and the iron parts, including the cores, the so-called neutral parts, pole-pieces, etc., as well as on the quality of the iron. As these are so very different in different machines, the constants obtained from one would be of little use in determining the size of other fields, except, perhaps, the constants given below for the intensity and amount of the magnetism of the field. A careful and systematic builder of dynamos will, in most cases, run the armature, when completed, with its own field magnet frame with temporary, removable coils of known number of turns, on the magnets, in order to find, by regulating the current in these temporary field coils, how many ampère-turns are required in the field to induce

the desired electro-motive force and current in the armature. From this number of ampère-turns any competent dynamo builder can readily calculate the proper windings of the magnets with all due accuracy.

Numerous theories of dynamo-electric machines have been advanced, but most of them are of greater interest to the mathematician and the physicist than to the practical dynamo builder. More attention seems to have been given to integrals, complicated abstruse fractions and formulæ for determining the presumably correct values for extreme impossible cases, than to simple, practical formulæ, by means of which tangible results may be obtained for the ordinary forms of machines. In some cases, formulæ based on physical theories have been suggested, which might have been of practical use if accompanied by the absolute and reliable values of certain constants usually represented by Greek letters, which enter as direct factors in the formulæ; but the values of these constants have either been omitted, or else have been given within such wide ranges that they cannot be used to any advantage in practice. They are sometimes of such a nature that they differ for each machine, thus necessitating the construction and testing of the machine before the values of the constants for calculating this particular machine may be determined; which is, to say the least, a very awkward way of applying a formula.

The writer has, therefore, set aside all abstruse theories of the dynamo, which have yet to stand the severe test of varied practical application, and has endeavored to deduce from the results given in the FRANKLIN INSTITUTE Report some constants, or values, which are in such a form that they may be directly applied to the calculations of the armature and some parts of the field. The deductions of these constants are based only on the well-known laws of induction and of mechanics, and as they are calculated from actual cases, they show what *is* done in practice, as distinguished from what *might be* done according to some theory, provided the theory is correct.

The results tabulated on the following page not only give the values which can be used in designing machines, but an attempt has also been made to determine the efficiency of separate parts; thus indicating under what circumstances the most advantageous proportions may be arrived at. Some proportions used in calcu-

lating the numbers given in the table were not contained in the report, and, therefore, had to be estimated, except where they were furnished to the writer by the kindness of the manufacturers. But the errors which may have been introduced by a slightly inaccurate estimate, are so small that they do not materially affect the results. Certain slight errors, or modifications, in some of the figures, or proportions, were not taken into account, as they cannot be determined; but they are of such a nature as to affect equally, or approximately so, all machines not differing too widely from the style of those tested. Among these is the self-induction of the armature; in well-built machines, like those tested in which the field is intense, the speed not too high, the number of commutator bars or coils large, and the number of windings per coil very small, the self-induction will be very small, and, therefore, the difference between the self-induction in these different armatures may be neglected.

In the table given below, the deduced constants have been accompanied by many of the proportions from which they were derived, as copied from the report, in order to show some of the principal proportions of the machines, and to give the conditions under which the constants have the values given. Numerous other values in the report might have been repeated here, but as it is presumed that any one can obtain the original report, they are omitted.* The values chosen for the deductions were taken from that one of the full load tests in which the current and potential were nearest to the values given by the makers as the best working load. (*See Table.*)

The following assumptions were made, as the accurate data for deducing the same were wanting. The speed in the Edison No. 4 machine was not given in the report; that given here is the speed at which the makers say the machine is to be run, and it is assumed that the speed must have been very nearly this in the test. The percentage of the whole circumference of the armature which is embraced by the pole-pieces was, in the Weston machines, assumed to be about eighty per cent., as this was the proportion in some arc-light machines of the same makers, which we understand have the same type of frame. The distance between the pole-piece

* For the full report, see this JOURNAL Supplement, November, 1885.

projections was determined from this in the Weston machines. In the Edison machines, the length of the pole-pieces and the armature-core, were deduced from the statement in the report giving the length of useful wire in a coil; it was assumed that by the term, "useful wire," was meant that which lies directly between the pole-pieces and the armature-core. In the Edison machines, it was assumed that the length of the armature-core was the same as that of the pole-pieces; in the Weston machines, this was the case.

As the electro-motive force induced in an armature is dependent upon the amount of magnetism passed through per second, the first questions in designing armatures are: How great may the velocity of the moving wire be? What must its length be? What must the intensity of the field be? etc.

The first of these, the velocity of the moving wire—commonly called the "conductor or inductor velocity"—depends on the distance of the active wire from the centre of the shaft, and on the number of revolutions of the armature. It may be calculated from the speed and the mean of the external diameter and the diameter of the core. In the accompanying table, the horizontal column *a* contains the external diameters, *b* the internal, and *c* the mean. From this mean value the circumference in feet was calculated, which, when multiplied by the speeds *d*, and reduced to seconds, gives the inductor velocities in feet per second, in column *e*. From these values, it is seen that the Edison armatures have a higher velocity of the moving wire; also, that it is preferable to obtain the high inductor velocity by making the diameter as great as practical, rather than to increase the number of revolutions, as will be seen by comparing the small, high-speed armature of the Edison No. 4 with the large one of the Nos. 10 and 20.

To obtain a constant by means of which the length of wire may be calculated, the total electro-motive force in volts which was generated, has been divided by the length of that part of the wire in which it is generated. This wire will be termed the "active wire," and, in these deductions, has been limited to that portion of the armature-wire which lies directly between the pole-pieces and the armature-core. Strictly speaking this is not quite correct, as some induction does undoubtedly take place in some parts of the wire lying at the ends of the armature, and also in some of the longitudinal wires which are not directly between the armature-core and

the pole-pieces; but the induction in both of these parts is presumably so small as compared to that in other parts, that it may be neglected, especially as the constants are to be applied to similar armatures, thus eliminating this error. The length of the active wires were calculated from the following proportions. Column *f* gives the number of coils or commutator-bars; *g* the number of turns per coil, and *h* the resulting total number of turns on the armature. The proportion of these coils which are active, is calculated from the distance between the pole-piece projections, column *i* and the circumference determined from the diameters in column *a;* the ratio of this is given in column *j*, in percentage of the circumference of the armature which is active; this will also represent the percentage of the number of windings which is active. Column *k* gives the length of a pole-piece in inches, which is the same as the length of the armature-core. From these the total length of the active wire in feet can be calculated, remembering that every turn of wire on the armature represents twice the length of the pole-piece; but as the two valves of the armature-wires are in multiple arc only half of this induces the whole electro-motive force. The total electro-motive force in volts, given in column *l*, is then divided by this half length of active wire in feet, giving the induction in volts per foot in column *m*. As this induction is slightly different in different positions of the wire with reference to the pole-pieces, these results give the mean value. They show that the induction is considerably higher in the Edison than in the Weston machines; also, that it is very nearly the same in all the Edison, and nearly the same in the three Weston machines.

These constants are dependent on the velocity of the inductor, for it is evident that if the latter were higher the induction would be increased. In order, therefore, to properly compare these constants, it is necessary to eliminate the velocity by dividing these figures by the velocities in column *e*, thus giving the volts per foot which would be induced (in the respective fields) at a uniform velocity of one foot per second. This is given in column *n*, and shows the number of volts induced per foot for an inductor velocity of one foot per second. These may be compared with each other as the velocities are the same. They show that at the same velocity the induction is better in the Edison than in the Weston machines; also that the values agree very closely for all the Edison,

except for the No. 4 machine, which may possibly be attributed to the number assumed for the speed, which was not taken in the test. For the Weston 6 M it is lowest, which is no doubt due to a less intense field.

The next question which naturally arises is, what was the intensity of the field in these machines? This may be determined as follows: We know that a volt is 100,000,000· times the unit of electro-motive force in the absolute system, and also that one absolute unit of electro-motive force is generated when a wire cuts lines of force at the rate of one per second; therefore one volt is induced if a wire cuts 100,000,000· lines of force per second. As we know the number of volts in one foot (column *m*) in these machines, and also the number of feet moved through in one second (column *e*), we can readily calculate the intensity which the field must have had to induce that number of volts in one foot. To illustrate this, suppose the velocity was one foot per second, and that the induction was one volt per foot at this velocity, then it is evident that the surface moved over by one foot of wire in one second; that is, one square foot, must contain 100,000,000· lines of force; or, if the induction was two volts per foot, there must be 200,000,000· lines if force in this space, as this number must have been cut per second to generate two volts. Dividing this number by 144, will give the mean intensity of the field in number of lines of force per square inch. These figures are given in column *o*. They show that with one exception, all the Edison machines have very nearly the same intensity of field; also, that it is somewhat lower in the Weston, especially in the 6 M machine; but, as will be seen, it is more economically obtained in this one, which indicates that it was probably not over saturated.

These values might also have been obtained by multiplying those in column *n* by 100,000,000· and dividing by 144, which is equivalent to multiplying them by 694,444. This will be seen by considering the principles of the deductions.

The total useful amount of magnetism in the whole field is evidently the intensity per square inch multiplied by the size of the field in square inches. As the intensity has been calculated from the amount of induction, the amount of magnetism thus obtained does not include the leakage of the magnetism; that is, those lines of force which are not cut by the armature-wire, it therefore

represents the useful magnetism only. The same lines of force which enter the armature at one pole-piece pass through it to the other pole-piece, and therefore the total number of lines of force is the intensity multiplied by the curved area of one pole-piece. These figures are given in column *p*, and are deduced from columns *o*, *a*, *j* and *k*. The magnetism increases with the number of volt-ampères which the machine generates.

This amount of magnetism is generated at the expense of a certain quantity of electrical energy in the field magnets. In order to get some approximate values for calculating roughly how much electrical energy will be required for generating in practice, a certain amount of magnetism in similar fields, the figures in column *p*, may be divided by the amount of energy in volt-ampères, that was required in these cases to generate the respective fields. In the report the figures in column *q*, are given, which are the energy in horse-power consumed in the fields. Unfortunately the resistance of the field without the regulator-box was measured only in one machine, so that the energy given here represents more than that used in the field-magnets themselves, thus introducing an error in all the deductions made from these values. It is presumed, however, that at full load the resistance in the box was not large, so that the error will probably be small. Reducing this energy in the field to volt-ampères and dividing it into the amount of magnetism, gives the number of useful lines of force generated per volt-ampère in the field. These are given in column *r*. As they depend on so many different proportions of the parts of the field and magnet-coils, and also in a measure on the armature, they might vary considerably for different types of field-magnets, and can, therefore, be used only in making rough preliminary calculations. They agree tolerably well for these fields, except for the Weston 6 M, which is evidently better proportioned than the others. As these figures are the ratio of that which is produced to that which is required to produce it, they may be said to represent the *relative* efficiencies of the different fields, in which sense they represent no absolute efficiencies, but serve simply to compare the efficiencies with one another. In the Edison machines, we believe wrought-iron alone is used, while in the Weston both wrought and cast iron were formerly used together, and we presume were used in these machines also. Possibly the iron in the field of the Weston

6 M machine is not over saturated, which may be the reason of its high efficiency of field.

The wires of the armature being wound repeatedly around it, pass through the same field a number of times, thus utilizing the same field in one revolution for successive inductions in the same wire. This number of turns, as given in column *h*, multiplied by column *j*, may therefore be said to represent the economic use of the field; it does not follow, however, that the best armature is the one having the greatest number of turns on it, as it is quite the reverse, other more important considerations, such as self-induction, sparking, etc., require that the number of turns of wire be as small as practicable.

In deducing the number of volts per foot, only the active wire was taken into consideration. In designing armatures it is therefore necessary to know what the proportion is between the active and the total length of the wire, for if the active length is determined first from the number of volts to be generated and the induction per foot, we must find what the total length is in order to determine the resistance or cross-section of the wire. This proportion of active to total length will evidently depend on several dimensions of an armature, and will, therefore, vary somewhat in different machines. For these machines it was determined as follows: column *s* contains the mean length of wire in one turn (where several smaller wires are in parallel they were considered as one); this multiplied by the number of coils in column *f*, gives the total lengths in column *t*; these divided into the active length in column *u*, give the percentage in column *v*. From the latter it will be seen that the economy of the wire is considerably better in the Edison than in the Weston, and that it is best in the Edison No. 20, the proportion between the length and diameter of the armature being greatest in this one. Most of the proportions from which these percentages have been deduced were given by the makers.

The density of the current in the armature-wire can be determined from the diameters of the wire in column *w*, the number of wires in parallel in a coil, in column *x*, and the total current in the armature in column *y*. Dividing the latter into the double area of cross-section of the wire in square mils, or the double sum of the cross-sections of the parallel wires, gives the number of square

mils per ampère in column *z*. These numbers vary somewhat with the current and with several other proportions; they should, therefore, be used only as a general guide, or for making preliminary calculations. The best method for determining the cross-section, is to find the resistance from the amount of energy which is allowed to be lost in the armature, which together with the total length of the wire as determined from the induction per foot, etc., gives the required cross-section.

The percentage of energy in the armature as taken from the report, is given in column *A*. Those in which the loss is least, Weston 6 WI and Edison No. 10, have the greatest area of cross-section of wire per ampère (column *z*), while the one in which the loss is greatest, Weston 7 M, has the least cross-section per ampère. This relation does not necessarily exist between them all, as the percentage of loss depends also on the induction per foot of wire, and therefore on the length of the wire.

The relative efficiencies of these armatures, as inductors, may be seen from the figures in column *C*. The efficiency as an inductor, apart from that as a converter of energy, is greater the larger the total amount of electrical energy induced (column *B*), and the less the amount of wire necessary to effect this induction; it may, therefore, be expressed by the quotient of the two. The figures in column *C* have, therefore, been calculated by dividing those in column *B* by the lengths in column *t* and by the double area of cross-section determined from columns *w* and *x*, the decimal point being changed to reduce them to a convenient form. By themselves these figures represent nothing, but by comparing them with one another they show which of the armatures are the best proportioned as inductors. They show that the Edison are considerably better proportioned than the Weston. The most efficient armature is, strange to say, the smallest one, Edison No. 4, which is no doubt due to its having the most intense field (column *o*), the highest speed (column *d*), and almost the smallest cross-section of wire per ampère (column *z*); this higher efficiency is, however, obtained at the expense of the efficiency of the field, as will be seen from column *r*; the useful commercial efficiency of the whole machine is, therefore, below the average, as seen from column *D*. Next to this armature, in point of efficiency as an inductor, is the largest one, Edison No. 20; its high efficiency is no doubt due to

the great intensity of field (column *o*), the very small amount of wire on the armature (column *t*), the relatively high inductor velocity (column *e*), the large proportion of active wire (column *v*), and the comparatively small cross-section of the wire, per ampère (column *z*). The field of this machine has next to the highest efficiency as seen in column *r*, and therefore, as might be inferred, the machine has the best useful commercial efficiency of all of those tested, as seen in column *D*. It may be interesting to mention here that in this machine which has the best efficiency, the armature-wires cut the field less frequently than in any other, as seen from the number of turns in column *h*.

Another proportion which may serve as a guide in designing armatures, is the relation between the outside diameter of the armature and the diameter of the core, or what amounts to the same thing, the percentage of the external diameter which is taken up by the wire on both sides. These figures are given in column *F*, and are obtained by dividing twice the depth of the windings given in column *E* by the external diameter in column *a*. This is greatest in the smallest armature, Edison No. 4, showing another disadvantage of small armatures; it is least and therefore best, in the Weston 6 M, which may partially account for the economic field shown in column *r*, as the latter depends on this non-magnetic space.

A few other proportions may be deducted from the data given. One of these is the proportion of the distance between the pole-piece projections and the distance between the pole-pieces and the armature-core. This proportion should evidently be as great as practicable. The figures are given in column *G* and are obtained from columns *i* and *E*, allowing about $\frac{1}{16}$ of an inch for clearance on each side. It is greatest for the largest machine, Edison No. 20, which may partially account for its economic field in column *r*. The reciprocals of one-half of the numbers in column *G* may be said to represent approximately the intensity of the leakage of magnetism, as compared to the intensity of the useful field at these places, as the lines of force have the choice of these two paths. But this will be only a rough approximation as a field is always more intense at points or sharp edges.

Another useful proportion is the relation between the length and diameter of the armature-core, as deduced from columns *k*

and *b*. It is given in column *H*, which shows by comparison with column *v* the advantage of a long armature. The Weston machines appear to be quite uniform in this respect, the length being almost twice the diameter.

Column *I* gives the relation between the length and diameter of the bearings.

Column *J* gives the electro-motive force between two neighboring commutator-bars. If this is great enough to maintain an arc across the insulation of the commutator-bars (about twenty volts) there is danger of starting the well-known flash, encircling the whole commutator, if the brushes should be misplaced sufficiently far to start the arc. It will be seen to be far within the limit of twenty volts in all of these machines.

The Edison Nos. 10 and 20 machines afford a good opportunity to compare two armatures of different lengths, but having all the other sizes and proportions alike, including the number of windings, size of wire, depth of winding, etc., only that in the one three wires are connected in parallel, and in the other, two; and that in the No. 20 the distance between the pole-piece projections is greater, making a difference of about ten per cent. in the number of active wires in favor of the smaller one. The chief gain of the long armature is seen in column *v*, in which it has the highest percentage, which partially accounts for the efficiency in column *C*, and the consequent commercial efficiency in column *D*.

The subject of self-induction of the armature was purposely omitted here, partly because insufficient data are given in the report to make any practical deductions regarding it, but principally because it is presumed that the self-induction in such armatures as these, with very few turns, is so small that it may be neglected in such rough values as have been deduced here. It is to be regretted that the resistance of the field-coils without the regulator-box was not measured in each case in order that the effect of the self-induction of the field-coils in increasing the apparent resistance could be calculated, and its relation to the number of pulsations or commutator-segments determined. In only one case was the field-resistance measured alone, and from this a calculation shows an increase of resistance of less than one-tenth of one per cent., and, therefore, probably less than the allowable error in measurement.

The fact that the Edison Nos. 5 and 20 machines gave way in the armature insulation will not lessen the value of these deductions to the designer of dynamos, as this was presumably a defect in the details of construction, and not in the proportion of parts.

The test of the Edison No. 20 machine was marked unofficial in the report because the preliminary run of ten hours was not on full load; but it is no doubt sufficiently accurate for the deductions of the approximate values in the table.

The deductions made here are not for the purpose of comparing the commercial value of these machines, as it has been shown in the tests that they are practically equal electrically. The most important consideration in this respect would be the cost of the machines and the cost of maintenance and attendance, which cannot be considered here.

For the benefit of any one not familiar with these machines, it may be added that they are all simple shunt machines, with cylinder armatures.

In conclusion, it may be said that the values deduced in the table may not be free from small errors, as great accuracy was not possible in all cases, owing to the want of some few detail dimensions; moreover, great accuracy has no particular value in such general deductions as these.

ANT-INHABITED PLANTS.—Hernandez, about the middle of the seventeenth century, described the stipular thorns of *Acacia cornigera* of Central America, into which certain ants eat, feed upon the pulpy interior, and live in the dwelling thus made. Such inhabited thorns grow larger and distorted, and the ants seem to pay for this hospitality by protecting the tree from other marauding insects. Two woody *Rubiaceæ* of Sumatra, were described in 1750, by Rumphius, as inhabited by ants. They are both epiphytic and attached to the host tree by a large tuberous base, which is cavernous and occupied by ants. The ants, by their irritating presence, cause the tuberous growth to enlarge, but the enlargement begins during germination, before the ants attack it, an instance of a plant preparing beforehand for expected guests. It is said that seedling plants which fail to become inhabited, perish. Dr. Gray, in a review, says that "it is most supposable that this extraordinary formation was acquired gradually; that the normally fleshy caulicle of the ancestral plant, made a nidus for an insect, developed under the disturbing stimulus somewhat as a gall develops until at length the tendency became hereditary, and the singular adaptation of plant to insect was established."—*Botan. Gazette, September and October, 1885.*

ADAM SCHAEFER'S COMPOUND FOR IMPROVING THE QUALITY OF STEEL.

By S. Lloyd Wiegand.

[*Abstract of a Communication, presented at the Stated Meeting of the* Institute, *held Wednesday, October 20, 1886.*]

This compound is the subject of letters-patent of the United States, numbered 341,173, and dated May 4, 1886. It consists of rosin, linseed-oil, glycerine and powdered charcoal, heated and intimately mixed in the proportions stated in the specification.

It is used by heating the steel to a clear red heat, and immersing and coating it in the compound, and the steel is afterwards re-heated and hardened in the usual manner by quickly cooling it.

Burned cast-steel is restored to its original condition, and the softer grades of steel acquire the properties of cast-steel by being treated as above stated. Tools made from Bessemer steel, which is incapable of being hardened, are, after treatment with this compound and hardening, capable of cutting cast-steel.

Tools so treated possess a greater durability than before, and are capable of cutting castings, which resist the best of ordinary cast-steel tools.

The grain of steel exhibited by fracture of tools so treated as compared with the same material before treatment shows a difference analogous to that between fine cast-steel and coarse or blistered steel.

The compound applied to gray castings and malleable iron-castings imparts a degree of hardness to them superior to ordinary case-hardening.

It is not attended in use with the unpleasant and deleterious fumes incident to case-hardening compounds, containing hydrocyanic acid, and is much less expensive.

Specimens of different materials in their normal state and also as treated with this compound and hardened were submitted, properly labelled, which conveyed a clearer conception of the effect than could be stated in language.

In order that the facility of application and its effect may be seen, a forge with fuel and bars of steel and other metal and a supply of the compound were submitted, by means of which

the members who felt inclined, personally tested it after the close of the meeting.

The compound has been introduced into practical use in many manufacturing establishments in this city with uniformly satisfactory results.

THE CONSTITUENTS OF COAL-TAR.*

BY ALFRED H. ALLEN F. I. C., F. C. S., of Sheffield, England.

The following table gives the names and formulæ of the numerous bodies, the pressure of which in coal-tar may be considered to be fairly well established, while other more volatile products of the distillation pass wholly into the gas. The presence in coal-tar of certain other compounds, which do not appear in the table, is strongly suspected; while of the constituents of the large fraction of coal-tar distilling between 240° and 270° C., and of the anthracene oils, much still remains to be learned. Of the composition of the residue or pitch still less is known.

The figures appended to the names of the constituents are their boiling points on the Centigrade scale. In the case of most of the compounds of very high boiling point, the melting points are also added.

The general composition of the various fractions obtained by the distillation of coal-tar will be regulated chiefly by the boiling points of their leading constituents, but the vapor-densities, vapor-tensions and relative abundance of the constituents of the tar also largely affect their behavior in the still. Thus, naphthalene is found in notable quantity in all the fractions from second light oils to anthracene oil, and is even deposited from the purified illuminating gas itself.

A MUSHROOM DEVELOPED IN HUMAN SALIVA.—M. Galippe, having filtered saliva by means of Pasteur's apparatus, the filtered saliva remaining undisturbed, saw appear at the lower end of the filter, not in contact with the saliva, a mushroom made up of tubes of mycelium and of spores. By the advice of Prof. Max Cornu, M. Galippe has cultivated this mushroom in Van Tieghem cells and has been able to prove that it was neither an aspergillus, nor a penicillium. This fungus, which has neither been described, nor drawn heretofore, belongs to the monilia family. M. Galippe proposes to give it the name of Monilia sputicola.—*Comptes Rendus, May 24, 1886.*

* From an advance-sheet of the new edition of the author's work on *Commercial Organic Analysis.* Vol. II.

TABULAR VIEW OF THE CONSTITUENTS OF COAL-TAR.

	Paraffin Series, $C_n H_{2n+2}$.	Olefin Series, $C_n H_{2n}$.	Crotonylene Series, $C_n H_{2n-2}$.	Benzene Series, $C_n H_{2n-6}$	Naphthalene and its Allies.	Hydrocarbons of other Series.	Nitrog
Constituents of heavy, dead, or Creosote Oils, distilling between 180° and 290° C. — Constituents of Crude Naphtha or Light Oils, distilling below 180°.	C_4H_{10}, Butane, + 1		C_4H_6, Crotonylene, 23				
	C_5H_{12}, Pentane, 38	C_5H_{10}, Amylene, 39	C_5H_8, Terene				
	C_6H_{14}, Hexane, 69	C_6H_{12} Hexylene, 67	C_6H_{10}, Hexoylene, 80	C_6H_6, Benzene, 81		C_6H_{12}, Hexahydrobenzene, 69	C_2H_3N, {Methyl isocyanide, 6 Acetonitrile,
	C_7H_{16}, Heptane, 97	C_7H_{14}, Heptylene, 96		C_7H_8, Toluene, 111		C_7H_{14}, Hexahydrotoluene, 97	$C_4H_{11}N$, Butamine, 75
						C_8H_{16}, Hexahydrometaxylene, 118	C_4H_5N, Pyrroli 130
	C_8H_{18}, Octane, 124			C_8H_{10}, Xylenes, {p. 136 m. 138 o. 142			
	(C_9H_{20}, Nonane, 146)					C_9H_8, Cinnamene, 145	
	$C_{10}H_{22}$, Decane, 167			C_9H_{12} {Mesitylene, 163 Pseudocumene, 166		C_9H_{18}, Nonone, 174	
				$C_{10}H_{14}$, Durene, 190	$C_{10}H_{12}$, Naphthalene tetra-hydride, 190		C_7H_7N, Aniline,
					$C_{10}H_{10}$, Naphthalene dihydride, 200–210		
					$C_{10}H_8$, Naphthalene, 218, m. p. 79		
					$C_{11}H_{10}$, Methylnaphthalene, {α 242 β. 241		C_9H_7N, Leucoline, {220 238
					$C_{12}H_{12}$, Dimethylnaphthalene, 252–270	$C_{12}H_{10}$, Diphenyl, 254, m. p. 70·5	C_9H_9N, Lepidine, {254 268
						($C_{12}H_{18}$, Acetnaphthene dihydride, 260)	$C_{11}H_{11}N$, Cryptidine, 272
						$C_{12}H_{10}$, Acetnaphthene, 285, m. p. 95	$C_{12}H_{13}N$, Tetracoline, 292
						$C_{13}H_{10}$, Fluorene, 295, m. p. 113	
						$C_{14}H_{16}$, Anthracene hexahydride, 290, m. p. 63	$C_{13}H_{15}N$, Pentacoline, 312
						$C_{14}H_{12}$, Anthracene dihydride, 305, m. p. 106	$C_{14}H_{17}N$, Hexacoline, 327
Constituents of Green or Anthracene Oils, distilling above 290°.						$C_{14}H_{10}$, {Phenanthrene, 340, m.p. 99 Synanthrene, m. p. 190 Anthracene, 360 m p. 213	$C_{15}H_{19}N$, Heptacoline, 347
							$C_{16}H_{21}N$, Octacoline, 362
						$C_{15}H_{12}$, Methylanthracene, m. p. 200–210	
						($C_{16}H_{14}$, Dimethylanthracene, m. p. 224)	
						$C_{16}H_{10}$, Fluoranthrene, m. p. 109	
						$C_{16}H_{12}$, Pseudophenanthrene, m. p. 190	$C_{16}H_{11}N$, Chry gene, m.p. 280–
						$C_{16}H_{10}$, Pyrene, 371, m. p. 140–142	$C_{18}H_{11}N$, Imi phenyl-naphth
						$C_{18}H_{12}$, Retene, 350, m. p. 99	m. p. 330
						$C_{18}H_{12}$, Chrysene, 440 (?), m. p. 249	
						$C_{22}H_{14}$, Picene, 519 (?), m. p. 338	
						$C_{24}H_{12}$, Benzerythrene, m. p 307	
Pitch.	C_nH_{2n+2}, Solid Paraffin						

TABULAR VIEW OF THE CONSTITUENTS OF COAL-TAR.

Benzene Series, C_nH_{2n-6}	Naphthalene and its Allies.	Hydrocarbons of other Series.	Nitrogenized
C_6H_6, Benzene, 81		C_6H_{12}, Hexahydrobenzene, 69	C_2H_3N, { Methyl isocyanide, 60 / Acetonitrile, 81
C_7H_8, Toluene, 111		C_7H_{14}, Hexahydrotoluene, 97 C_8H_{16}, Hexahydrometaxylene, 118	$C_4H_{11}N$, Butylamine, 75 C_4H_5N, Pyrroline, 130
C_8H_{10}, Xylenes, { p. 136 / m. 138 / o. 142		—	
C_9H_{12} { Mesitylene, 163 / Pseudocumene, 166		C_8H_8, Cinnamene, 145	
		C_9H_{14}, Nonone, 174	
$C_{10}H_{14}$, Durene, 190	$C_{10}H_{12}$, Naphthalene tetra-hydride, 190 $C_{10}H_{10}$, Naphthalene dihydride, 200–210 $C_{10}H_8$, Naphthalene, 218, m. p. 79 $C_{11}H_{10}$, Methylnaphthalene, { α 242 / β 241 $C_{12}H_{12}$, Dimethylnaphthalene, 252–270		C_7H_7N, Aniline, 182 C_9H_7N, Leucoline, { 220 / 238 $C_{10}H_9N$, Lepidine, { 254 / 268
		($C_{12}H_{16}$, Acetnaphthene dihydride, 260) $C_{12}H_{10}$, Diphenyl, 254, m. p. 70·5	$C_{11}H_{11}N$, Cryptidine, 272
		$C_{12}H_{10}$, Acetnaphthene, 285, m. p. 95 $C_{13}H_{10}$, Fluorene, 295, m. p. 113	$C_{12}H_{13}N$, Tetracoline, 292
		$C_{14}H_{16}$, Anthracene hexahydride, 290, m. p. 63 $C_{14}H_{12}$, Anthracene dihydride, 305, m. p. 106	$C_{13}H_{15}N$, Pentacoline, 312 $C_{14}H_{17}N$, Hexacoline, 327
		$C_{14}H_{10}$, { Phenanthrene, 340, m.p. 99 / Synanthrene, m. p. 190 / Anthracene, 360 m p. 213	$C_{15}H_{19}N$, Heptacoline, 347 $C_{16}H_{21}N$, Octacoline, 362
		$C_{15}H_{12}$, Methylanthracene, m. p. 200–210 ($C_{16}H_{14}$, Dimethylanthracene, m. p. 224) $C_{15}H_{10}$, Fluoranthrene, m. p. 109	
		$C_{16}H_{12}$, Pseudophenanthrene, m. p. 150 $C_{16}H_{10}$, Pyrene, 371, m. p. 140–142 $C_{18}H_{18}$, Retene, 350, m. p. 99 $C_{18}H_{12}$, Chrysene, 440 (?), m. p. 249 $C_{22}H_{14}$, Picene, 519 (?), m. p. 338 $C_{24}H_{12}$, Benzerythrene, m. p 307	$C_{16}H_{11}N$, Chrysogene, m.p. 280–290 $C_{16}H_{11}N$, Imidophenyl-naphthyl, m. p. 330

Leucoline bases.

Nitrogenized Bodies.			Oxygenated Bodies.	Sulphur Compounds.
		H_3N, Ammonia		(C_2H_6S, Methyl sulphide, 41)
$_3N$,	Methyl isocyanide, 60 / Acetonitrile, 81			($C_4H_{10}S$, Ethyl sulphide, 91)
			C_2H_6O, Alcohol, 78	(C_2H_6S, Ethyl sulphydrate, 36)
				CS_2, Carbon disulphide, 46
$_{11}N$, Butyline, 73			H_2O, Water, 100	C_4H_4S, Thiophene, 84
$_5N$, Pyrroline,				C_5H_6S, Thiotolene, 113
		C_5H_5N, Pyridine, 116	$C_2H_4O_2$, Acetic acid, 119	C_6H_6S, Thioxene, 137
		C_6H_7N, Picoline, (*o.* 124, *m.* 140)		
		C_7H_9N, Lutidine, 154		
		$C_8H_{11}N$, Collidine, 170		
$_7N$. Aniline, 182			C_6H_6O, Phenol, 182	
		$C_9H_{13}N$, Parvoline, 188	C_7H_8O, Cresols, (*o.* 186, *m.* 198, *p.* 201)	
			$C_9H_{10}O$, Xenol, 212	
		$C_{10}H_{15}N$, Coridine, 211		
			($C_9H_{12}O$, Cumenol)	
$_7N$, coline,	220 / 238	$C_{11}H_{17}N$ Rubidine, 230		
$_9N$, idine,	254 / 268	$C_{12}H_{19}N$, Viridine, 251	$C_7H_6O_2$, Benzoic acid, 250, m. p. 120	
$_{11}N$, Cryptine, 272				
$_{13}N$, Tetraine, 292			$C_{10}H_8O$, Naphthol, (α. 278–280, β. 285–290)	
$_{15}N$, Pentaine, 312				
$_{17}N$, Hexaine, 327				
		$C_{12}H_9N$, Carbazol, 355, m. p. 238		
$_{19}N$, Heptaline, 347			$C_{14}H_{10}O$, Phenanthrol / Anthrol	
$_{21}N$, Octaine, 362		$C_{13}H_9N$, Acridine, 360, m. p. 127		
$_{11}N$, Chryso-e, m.p. 280–290				
$_{11}N$, Imido-enyl-naphthyl, p. 330				

Leucoline bases. Pyridine bases.

THE LUMINIFEROUS ÆTHER.

BY PROFESSOR DE VOLSON WOOD.

I supposed that I had reported Herschel so faithfully that there was no occasion for criticism, and I now infer, from the statements of Prof. Chase in the last October number of this JOURNAL, that had I used "hypothesis" for "*assumption*," he would have been satisfied. I waive this point without discussion. The "questionable as to the correctness of the facts," that writer has misapplied. In the August number, he referred to "results which I supposed were new." As I had not confided that secret to any person, the correctness of the statement might be questioned. On page 300, he continues: "Prof. Wood says that 'in a pound of the æther there is 100,000,000,000 times the kinetic energy of a pound of air.' This discrepancy arises from his inadvertently omitting one cipher and making a rougher approximation than Herschel." This is an error. The "inadvertence" belongs to Prof. Chase. What I said was "in a pound of æther there is some 100,000,000,000," etc. I inserted the word "some" on purpose that I might agree approximately with those who should make a microscopic examination of the figures; knowing that in an off-hand statement of this kind, in a number of such magnitude, it was of little consequence whether the left hand figure were seven, eight, or even nine times as large. The word "some" inserted as it was, shows that it was only a rough approximation.

Then he proceeds to say: "Wood's result being 8×10^{11} nearly, while Herschel's is 811,801,000,000." I supposed that I must have given the result apparently attributed to myself, but I am unable to find it in my article; and I believe that it is a result of Prof. Chase's computation, and if so, why did he not make it agree with Herschel's. I do not, however, object to the value given, for I get 814,400,000,000, by using 186,300 miles as the velocity of light per second. Moreover, is not Herschel's result "8×10^{11} nearly?" But these hair-splittings belittle the subject.

For the benefit of those who are interested in the subject, but have not time to study it, I will give a very brief statement of the

manner in which the problem has been attacked. Herschel, on the hypothesis that the æther is as dense as air at sea-level, estimated the ratio of the elasticity to the density of the æther to be 17×10^{12} pounds per square inch. He made no estimate as to the actual density, nor actual tension, but gave it as his opinion that its tension was very great. Thomson, on the assumption that the mean displacement of a particle from its normal position was one-fiftieth of a wave length, found a corresponding density of the æther. Maxwell, in his article on "Ether" in the *Encyclopædia Britannica*, IX edition, on the hypothesis that the displacement was $\frac{1}{100}$ of a wave length, obtained his estimate of the density and elasticity. Other writers have obtained results on this principle.

Preston, on the hypothesis that the tension of æther exceeds the force necessary to separate the atoms of oxygen and hydrogen in a molecule of water, and that the probable inferior limit of this tension was 500 tons per square inch, computed its corresponding density.

In my solution no such arbitrary assumption or hypothesis was made. It was simply assumed that the æther conformed to gaseous laws, and the two constants—the solar constant and the velocity of light—were the data for making the equations numerically determinate in regard to the elasticity and density. This and other processes and results given in my article have not been attacked so far as I am aware.

CONFIRMATION OF CHEMICAL PREDICTION.—Supposing the new element, Germanium (Gr) to be the Eka-silicium, predicted by Mendelejeff, to lie between silicon and tin, Lecoq de Boisbaudran (who, even prior to Mendelejeff, had surmised the existence of an element with atomic weight 72·28) now gets 72·27 by spectroscopic observation of a fragment too minute for other treatment. He assumes that the lines $\lambda = 412{\cdot}9$ and $\lambda = 389$ for Si, are homologous with the conspicuous lines 468·0, 422·6 for Gr, and with 563·0, 452·4 for Sn; and that the lines 395·2, 410·1, 430·6 for Al, Ga, In, are homologues; and he uses the atomic weights 28·0, x, 118·0 for the triplet Si, Gr, Sn, and the weights 27·5, 69·9; 113·5 for the triplet Al, Ga, In. Then for each triplet of wave-lengths, or of atomic weights, he finds the quotient, second difference ÷ leading first difference; and he determines x so that these quotients for atomic weights in the two series may be to each other as are the corresponding quotients for wave-lengths. He adds that by the same method he once predicted the weight of Ga within one-sixtieth of one per cent.—*Comptes Rendus, June 7, 1886.*

PHOTOGRAPHING WITH PHOSPHORESCENT SUBSTANCES.

At the stated meeting of the INSTITUTE, November 17th, Mr. Frederic Ives read a preliminary communication on this subject, giving the result of some investigations, which he had undertaken at the suggestion of Dr. Wahl.

After making a photograph of a street scene by exposing in the camera for thirty seconds, a tablet coated with Balmain paint and then placing it in contact with a photographic sensitive plate for about the same length of time, Mr. Ives made exposures on the lime-light spectrum, to determine to what kind of light the tablet was sensitive. He found that phosphorescence was produced only by exposure to the violet rays, and that the light given out by the excited tablet was chiefly the indigo-blue, to which photographic sensitive plates are more sensitive than to any other color. But he also observed that a tablet exposed to sunlight, and then given a rest of several hours in total darkness, in a cool place, became quite sensitive to the dark heat rays at the opposite end of the spectrum, which caused a temporary exaltation and corresponding rapid exhaustion of the feeble phosphorescence remaining in the tablet. He succeeded in obtaining strong photographic negatives showing this action, and concluded that by this means it would be possible to obtain camera photographs of perfectly dark objects which radiated or reflected sufficient heat, provided that the lenses used were capable of freely transmitting such dark heat rays.

M. Ch. Zenger, in a recent communication to the French Academy of Sciences, asserted that he had obtained a photograph of towers and other objects at midnight on a dark night, by the aid of a phosphorescent tablet, and attributed the result to the action of dark "actinic" radiations, which he supposed that the objects gave out at night. Mr. Ives pointed out that, as the tablet proved to be insensitive to such rays, Zenger's explanation was certainly incorrect. Mr. Ives's investigation had proved that bodies intensely heated by exposure to the sun during a hot summer day might, under certain conditions, be photographed at night by the action of heat which they radiated,—but the method did not seem sensitive enough to give such results with glass lenses, and he thinks

Zenger's photograph may have been due to unsuspected feeble phosphorescence remaining from a previous exposure in daylight, several hours or even a day or two before.

Mr. Ives offered explanations of other phenomena observed by M. Zenger, and concluded by claiming to be the first to discover that photographs of dark objects may be made in the camera, by the action of heat which they radiate or reflect.

BOOK NOTICES.

THE LUMINIFEROUS ÆTHER. By De Volson Wood, C. E., M. A. Van Nostrand's Science Series, No. 85. Price, $0.50. Reprinted from the *Philosophical Magazine*, with additions. New York. 1886.

In view of the importance which has been assumed by molecular hypotheses, in scientific investigation, the vague ideas which are still entertained respecting molecular æther—so vague as to have led to variations of more than a million-fold in estimates of æthereal density—are a reproach to science. This reproach, our author has, very commendably and very judiciously, endeavored to overcome, and, considering the number of novel questions he has had to meet, he has been very successful.

There have been too many printer's errors, and the very serious collateral misunderstandings, in which others had already given a wrong bias to popular interpretations, have already been noticed in the JOURNAL; but his proposal on pp. 9–11 is admirably and satisfactorily stated.

He asks, p. 10–11, "Can the kinetic theory, which is applicable to gases in which waves are propagated by a to-and-fro motion of the particles, be applicable to a medium in which the particles have a transverse movement, whether rectilinear, circular, elliptical, or irregular? In favor of such an application, it may be stated that the general formulæ of analysis by which wave-motion in general, and refraction, reflection, and polarization in particular, are discussed, are fundamentally the same; and in the establishment of the equations the only hypothesis in regard to the path of the particle is—it will move along the path of least resistance. The expression $V^2 \propto \varepsilon \div \delta$ is generally true for all elastic media, regardless of the path of the individual molecules. Indeed, granting the molecular constitution of the æther, is it not probable that the kinetic theory applies more rigidly to it than to the most perfect of the known gases?"

This is the hypothesis of Herschel, the only one which has ever led to any numerical verification, or ever received any practical confirmations. It has given, correctly, the elasticity which the æther would have at the same density as the atmosphere, from which it is easy to find the elasticity for Thomson's or any other supposed density. If Prof. Wood will examine all the best density-estimates and give us one which is as accurate as Herschel's combined elasticity-density estimate, he will restore the credit which he helped, wrongly but unintentionally, to take from Herschel, and he will help remove all claim

to scientific value from two solutions which are based on the same data, and one of which is more than 1,600,000 times as great as the other.

On page 5, of his brochure, Prof. Wood says: "Beyond these facts, no progress can be made without an assumption." There is no assumption in saying that action and reaction must always be equal. The action of radiation from every point of the sun, produces a wave-velocity in the æther which is the same as that of light, and the rotary time-integral of the cyclical reactions of solar centripetal gravitation also produces a velocity, at every point, and for the whole cycle, equivalent to the velocity of light. The sum of the impulses communicated by the acceleration of solar-superficial gravitation during any cyclical rotation on its axis ($g\,t$), equals twice the average velocity which would be communicated if the efficiency of all the impulses were retained $\left(\frac{g\,t}{2}\right)$. The sum of the wave-disturbances of solar radiation during a solar rotation on its axis could have no more fitting representative reaction than the sum of the reactionary gravitating accelerations on the oscillating particles themselves.

If we represent the time of solar rotation by t, solar-superficial gravitating acceleration by g, and the velocity of light by v_λ, this perpetual equality of acceleration and re-acceleration may be represented by the equation $\frac{g\,t}{2} = v_\lambda$. By the law of conservation of angular momentum, $t \propto r^2$, while $g \propto \frac{1}{r^2}$, so that $g\,t$ is constant at all stages of nebular condensation, and the dependence of solar rotation, planetary revolution and luminous radiation upon a single energy at the centre of our solar system has been recorded in the heavens since the day of the great *fiat*, "Let there be light." P. E. C.

THE PREVENTION OF FIRE. CHIEFLY WITH REFERENCE TO HOSPITALS AND OTHER PUBLIC INSTITUTES. By William P. Gerhard, C. E. Published at 6 Astor Place, New York. Price, 60 cents.

This brochure groups well various good suggestions often previously reiterated by others. We like the author's earnest advocacy of "slow burning" construction in buildings, since no mode is absolutely fire-proof. He counsels the avoidance of vacant spaces, those between joists, in partitions and under stairways, to be filled with scoria or plaster of Paris, and outside covered with wire netting and plastered. He is not explicit as to floors, but we presume he means hardwood, such as oak; nor does he state, if it be adopted, how the extra weight of a scoria-filled roof is to be best supported. He advocates, most correctly, incombustible coverings for roofs, but we are surprised that he classes "tar and gravel roofs" among those which are very good. As to supporting heavy weight, we think he should have noticed the advantage of iron or wooden pillars (preferably the latter), *sheathed thickly in cement.* What he states about hidden dangers in flues and other flimsy kind of construction is very true, and good attention is given to proper heating, illumination, laundry fixtures, water and appliances for fire extinguishment. If the warnings of this pamphlet were well heeded, the number of fires would undoubtedly be largely diminished. N.

UNERH. JAHRGANG NO. 20, of the "Berichte der d. chem. Gesellschaft," has just been received from R. Friedländer & Sohn. Berlin.

This is the most interesting number for a long time, and was doubtless edited with especial reference to the late meeting of the Physicians and Naturalists in Germany's capital. Its 51 pages (S. 3517–S. 3568, inclusive) are full of the varied notes and Abhandlungen, which have given its very definite character to this publication. The many-sided nature of its contents may be gathered from the following titles: "Die Bedeutung der Alcohol-Entziehung," "Ueber den kritischen Druck," "Die Synthese des Cognacs," "Versuch einer chemischen Theorie des Lichtes," "Vereinfachung eines der meist gebrauchten Laboratoriumsgeräthe," "Ueber den Zusammenhang zwischen Constitution und Krystallform," "Ueber einen einfachen Thermo-regulator," "Vorlesungsversuch" (an ingenious method of showing to a class the blue color of water), "Eine neue physikalische Constante und ihre Verwendung zu Moleculargewichtsbestimmungen," "Zur chemischen Nomenclatur," "Zur Constitution des Benzols" (this article includes an entirely original graphic representation of the benzol hexagonal molecule); a sharp retort to H. C. Bissig's supposed correction of a melting point; a joint article (A. Riese and P. Böhm) on "The Isolation of one of the Sulpho-chlor. Benzol Compounds," "Ueber die Drehkraft in der Natur," etc., "Zur Kenntniss der Zuckersäuren." Among the referate we may single out the discovery of a new metal, to which the name "Pankowium" has been given. It is thought by the discoverer, Herr Heuschrecker, that two other new elements to which he gives the names "Gesundbrunnium," and "Viehhofium," are associated with the first in small quantity." "A New Air-Pump," "Reasons for Belief in the Hour-Glass Form of certain Molecules."

Under organic chemistry are "The Condensation Products of Aldehyde," "Stanislausine," the odoriferous constituent of *Cimex lectularius*, by E. Stanislaus, and a supplementary paper by B. Neinmeyer on "The Difficulty if not Impossibility of Purifying the Body of Stanislaus," on "Thiophenomenaldehyde," the foulest smelling substance of modern organic chemistry. Items in physiological and analytical chemistry follow. Under the head of "Patents" is one of a new black dye, said to have been taken out by a Philadelphian. It consists of "gefälltes Palladiumiodid, Gastheer, Blauholzextract, Russ, Anilinschwarz, Tinte und Schuhwichse," which being mixed in the proportion of their molecular weights, are kept twelve hours at a temperature of 385°. An English patented sparkling wine, called Xylomousseux, is made by distilling shavings and potato skins, saturated with molasses, and charging with carbonic acid at high pressure. The remaining pages of the number are devoted to chemico-literary articles in honor, no doubt, of the distinguished foreign visitors of the great scientific gathering.

A MANUAL OF LITHOLOGY. By Edward H. Williams, Jr., E. M., Professor of Mining Engineering and Geology, Lehigh University, South Bethlehem, Pa.

This little book of 135 pages, 5¾ x 3¾ inches, only proposes "to combine

a thorough knowledge of the elementary portion of the subject with a brief account of the principal rocks and a ready method for their determination." To do this, it combines, to a large extent, the definitions as given in Lawrence's *Cotta* ("Rocks Classified and Described"), with the tabular method of mineral description in Frazer's *Weisbach* ("Tables for the Determination of Minerals"), except that here the columns and the descriptions are independent. The little work is divided into a first part, consisting of a chapter on "Lithology," and one on "Mineralogy;" a second part, with a chapter on "Definitions," etc.; another on "Primary Rocks," considered as acid vitreous, basic vitreous, acid crystalline, intermediate crystalline and basic crystalline; a third chapter on "Sedimentary Rocks" of (1) the sedimentary and (2) the fragmental class; a fourth chapter on the "Metamorphic Schists," to wit: the acid series (feldspar and quartz group), and the basic series, (1) (chlorite, talc and hornblende group), and (2) argillaceous group; a fifth chapter, including the serpentine, garnet, olivine, coal, guano, iron and mineral groups. Part III contains a scheme for determining rocks and a classification. This latter is a courageous attempt to put into six pages enough definitions to enable the learner to classify and determine a rock.

The little book is well printed and proof-read, though the paper is a trifle thin. In some cases, the words of Lawrence's *Cotta* are slightly altered or transposed, as in the definition of texture, p. 2; syenite, p. 63, when "pyroxene" is introduced, an association that would have made Von Cotta frown, etc., etc., and in some cases, the author has bodily transposed the defining phrases from one thing to another, as in his enumeration of the kinds of structure which are taken from Lawrence's varieties of "Texture," even to the "Spherulitic or *Globuliferous*," which Lawrence has left as a little monument of kakophony to himself.

In some few places, carelessness appears, as in his note on page 2, where he applies the term "magma" to the homogenous or amorphous matrix of crystals, which is exclusively employed for a precipitate or mass of crystals, or mixture of substances in a pulpy or pasty state. (See Watts' *Dictionary of Chemistry*.)

But independently of a few blemishes of this kind, the little pocket companion will be of use to practical miners and beginners in rock study, in spite of the fact that it leaves out of account all that the modern Zirkels and Rosenbusches have done. It were to be wished, though, on this account, that if the order of sequence in Von Cotta was to be abandoned, some of the modern systems had been adopted. Lossen's would have been a good one for the eruptive rocks, because it has been adopted by the International Geological Congress.

P. F.

ARC AND GLOW LAMPS. A PRACTICAL HAND-BOOK ON ELECTRIC LIGHTING. By Julius Maier, Ph. D., Associate of the Society of Telegraph Engineers and electricians. New York: D. Van Nostrand.

This volume, as one of the excellent "Specialist's Series," is to be commended for its faithfulness to the technical topics treated. It is divided into

four parts, the first of which contains descriptions of methods and instruments used in practical measurements and also quotes results of some of the recent measurements for determining the light-efficiency of dynamos. The arrangement of leads and the special devices for regulation both in arc and glow lamp installations, and the cost of electrical illumination are also here discussed.

Part second treats of arc lamps, their construction and efficiency. All the well-known and some little-known lamps are figured and discussed in their mechanical and electrical details. In treating of the light-efficiency of the various arc lamps, the author gives the results of the tests made in Paris, Vienna and Philadelphia. The latter tests made under the direction of Professor Wm. A. Anthony, Chairman of the Section on Arc Lamps of the Board of Examiners of the FRANKLIN INSTITUTE, are referred to *in extenso*, and the tabular results reproduced. Part third is devoted to the construction and efficiency of glow lamps. It is a compend of the experimental tests made on incandescence lamps. Of especial interest are the references to the experiments of Siemens and Halske on unimproved and improved filaments and to the much mooted relations between efficiency and life in these lamps. The concluding part reviews the application of electric lighting to light-houses, ships, collieries, military operations, railway trains, theatres, photography, the microscope and surgical operations. M. B. S.

TOWN GEOLOGY: THE LESSON OF THE PHILADELPHIA ROCKS. By Angelo Heilprin, Professor of Invertebrate Palæontology at, and Curator in Charge of, the Academy of Natural Sciences, Philadelphia.

This book is a handsomely printed and edited, large margin octavo of 134 pages of very readable reading-matter, adorned with very pleasant sketches in the text and fine plates.

Prof. Heilprin adopts that happy style of presenting the scientific facts with which he is familiar, which Hugh Miller and Huxley and Tyndall, and many others have done much to associate both with sound and with unsound science. His points of view are original and striking, as is the manner of his introducing the subject in hand. After thirty-two pages of general observations, he takes up the rocks of the Schuylkill and Wissahickon, and follows this by a chapter on the oldest patch of land. It is highly to the credit of Prof. Heilprin that while he tells us much that is interesting, he avoids committing himself to any one of the numerous hypotheses in regard to the age of these rocks, which one and all are as yet in unstable forms. It would have been most natural to do this, and it must have been difficult to avoid it, which only shows that the more credit is due to Prof. Heilprin for his skill and tact. We venture to say that the average reader, beguiled from one page to another by the agreeable and profitable lines, will not note the absence of this dogmatizing. The same judicial impartiality is observable in the maps at the end of the book, where the rocks in dispute are modestly called Philadelphia schists and gneisses, a characterization in which all can agree. The plates are well engraved, and the book is creditable to author and printer.

P. F.

THEORY OF MAGNETIC MEASUREMENTS. WITH AN APPENDIX ON THE METHOD OF LEAST SQUARES. By Francis E. Nipher, A. M., Professor of Physics in Washington University; President of the St. Louis Academy of Sciences. New York: D. Van Nostrand. 1886.

This little volume of scarcely 100 pages octavo, is an excellent *résumé* of the theory necessary to practical determination of the three magnetic elements—declination, dip and intensity. As is well known, Professor Nipher some years ago made a comprehensive magnetic survey of his own state—Missouri—in such a manner as to do credit to his interest in this department of science. The experience thus gained, and the knowledge of details of methods and instruments, having been frequently requested from the Professor, he has very properly made reply in the form of the present little book, so as "to furnish information relating to the practical details of a magnetic survey."

In order to be able to present an explanation of proper methods of reduction of the observations, the author also finds it convenient to give a brief appendix on the method of least squares. The volume is characterized by accuracy and thoroughness, and cannot fail to be of service to those who wish to become acquainted with good method in this important field. Practical examples are worked out according to the mathematical theory developed, and, very appropriately, all is presided over by a fine frontispiece of Carl Friedrich Gauss, to whose genius the science of terrestrial magnetism chiefly owes its existence.

M. B. S.

COMETARY SPECTRUM.—Trépied, at Algiers, April 7th, gets a continuous spectrum for the Fabry comet, together with three bright hydro-carbon bands that are strongest in the spectra of coma and tail, though the nucleus is very brilliant and star-like. The same thing is observed by MM. Thollon and Perrotin, at Nice. Thus, as with Encke's comet last year, the light from luminous gases predominated.—*Comptes Rendus, May 3, 1886.*

LIQUID O AND N.—Wroblensky finds for the density of liquid oxygen, 0·6 at —118° C., the critical temperature; 1·24 at —200°; and 1·212 + ·00428 T — ·0000529 T^2 for intermediate temperatures, T being the absolute temperature. This gives the atomic volume less than fourteen, not sixteen, as Dumas thought. For nitrogen, Wroblensky gets the densities ·4552, ·5842, ·83, ·866 at the respective temperatures —146°·6, —153°·7, —193°, —202°, the freezing-point being —203°. Thus, the atomic volume is nearly 15·5. Air, at these low temperatures and pressures, behaves like a mixture of unequally-volatile liquids: its composition varies with temperature and pressure, and its density conforms to the theory for mixtures. As Wroblensky employed only slight or moderate pressures, Amagat, who last year got for oxygen a density above 1·25 by 4,000 atmospheres' pressure at 17° C., suggests that cold and pressure combined may so far increase the above densities that, while the atomic volumes of S, Se, Te may be equal, that of O may be just half as great.—*Comptes Rendus, May 17, 1886.*

Franklin Institute.

[*Proceedings of the Stated Meeting, held Wednesday, November 17, 1886.*]

HALL OF THE INSTITUTE, November 17, 1886.

Vice-President CHAS. BULLOCK, in the Chair.

Present, 109 members and eleven visitors.

Additions to membership since the previous meeting, eight.

By direction of the Chairman of the Committee on Science and the Arts, the Secretary reported the Committee's recommendation for the award of the *Elliot Cresson Medal* to ROBERT H. RAMSEY, of Philadelphia, for his "Car-Transfer System and Apparatus;" and to THADDEUS S. C. LOWE, of Norristown, Pa., for his "Improvements in the Manufacture and Applications of Water-Gas for Heating and Lighting."

The recommendations were severally approved, and the Secretary was directed to take the usual measures to carry them into effect.

A report of progress was presented on behalf of the Special Committees having in charge (1.) the preparation of a plan for a "State Weather Service," and (2.) the consideration of Mr. Paul La Cour's protest against the award of the *Elliot Cresson Medal* to PATRICK B. DELANY.

MR. FRED'K E. IVES, of Philadelphia, presented a preliminary communication on the "Application of Phosphorescent Substances to Photography." An abstract of this communication appears in this impression of the JOURNAL.

The Secretary's report embraced remarks on the "Future Water-Supply of Philadelphia," on the "Progress of the East-Side Connecting Railway," and on some of the "Indirect Advantages of Natural Gas."

Adjourned. WM. H. WAHL, *Secretary*.

DRUIDICAL REMAINS IN PERU.—M. Daubrée offers, in the name of M. Habich, Director of the School of Civil Constructions and Mines, of Lima, the fifth volume of *Anales de Construcciones Civiles y de Minas*. An article by M. Chalon, on "The Buildings of Ancient Peru," is worthy of notice and of universal interest. To the primitive epoch, succeeding the pre-historic, belong constructions laid with rough blocks, sometimes of very great dimension. It is very remarkable that these monuments offer the greatest resemblance to those which are known in the old world, particularly in France, under the names of raised stones, menhirs, cromlechs, dolmens and the general term druidical. They are very numerous and are spread through all parts of the territory of Peru. According to the author, they appear to have a religious significance and to have served for sepulchre and sacrifice.—*Comptes Rendus, May 24, 1886.*

THE "NOVELTIES" EXHIBITION OF THE FRANKLIN INSTITUTE, 1885.

ABSTRACTS OF REPORTS OF THE JUDGES.

(*Continued from page 400.*)

GROUP 12*a*.—GAS MANUFACTURE FOR ILLUMINATING AND HEATING, AND APPARATUS THEREFOR, INCLUDING COAL- AND OIL-GAS AND NATURAL-GAS APPLIANCES.

Judges:—Griffith M. Eldridge, Wm. H. Greene, L. B. Hall, Wm. D. Marks, G. M. Ward, Moses G. Wilder, Wm. H. Wahl, *Chm.*

The exhibits referred for examination embraced the following:

(1.) Exhibit of The Lowe Manufacturing Co., of Norristown, Pa.—a comprehensive exposition of the manufacture and applications of Water-Gas.

(2.) Exhibit of the Pennsylvania Globe Gas Light Company, of Philadelphia.—The Royal Gas Machine.

(3.) Exhibit of Rand & Harmer, of Philadelphia.—The Victor Gas Machine; and

(4.) Exhibit of Jas. P. Wood & Co., of Philadelphia.—The Globe Gas Generator.

Air-Carburetting Machines.—These machines operate on the principle of saturating atmospheric air with the vapors of the lighter products of petroleum; the vapor-laden air, possessing high illuminating qualities, passes to the burners through distributing pipes, after the manner of utilizing coal-gas.

There are two types of these machines, both of which are represented at the Exhibition.

In one, the air is forced by mechanical means, to pass over a body of gasolene, and to charge itself with the illuminating vapors at the ordinary temperature. This class comprises the so-called air-blowing machines.

In the other, artificial heat is employed to effect the vaporization of the gasolene. This class comprises the so-called caloric machines.

The machines exhibited by *Rand & Harmer* and the Pennsylvania Globe Gas Light Company, belong to the first-class; that exhibited by Jas P. Wood & Co., belongs to the last.

RAND & HARMER, PHILADELPHIA.

The Victor Gas Machine.—This machine has an air-pump, consisting of a meter-wheel, which is actuated either by weights and pulleys, or (in the specimen examined by the undersigned) by an overshot water-wheel. When the machine is in operation, the pump delivers an automatically-controlled current of air through an air pipe to a carburetter, in which an extended evaporating surface of gasolene is presented to it. The air is thus "carburetted," and in this condition passes to the burners.

The air-pump and carburetter are made of copper and brass, except the frame, which is of iron. The carburetter is made of sufficient capacity to hold a six-months' or year's supply of gasolene.

In the machine exhibited, the controlling mechanism by which the quantity of gas made is automatically regulated by the amount being consumed, consists of a floating air-chamber—on the principle of a gas-holder—and which is placed on top of the air-pump. A lever is attached, at one end, to a vertical stem rising from the floating air-chamber, and at the other, to a stop-cock in the supply-pipe, which furnishes the overshot wheel, above-named, with water. When the gas consumption increases the air-chamber descends, the water-supply stop-cock is opened wider, a correspondingly larger quantity of water is delivered in the buckets, the water-wheel and meter-wheel on the same shaft are caused to revolve more rapidly, and the result will be the delivery of a correspondingly larger quantity of air to the carburetter and of carburetted air to the burners. The reverse action takes place when the consumption of gas is reduced, and, when the lights are all shut off, the stop-cock is entirely closed, and the machine comes to rest.

The makers state that the carburetter containing the gasolene is placed in the ground, at any required distance from the building to be lighted, and that they provide it with a safety-valve, as a safeguard against undue pressure either way.

They claim for it, simplicity of design; entire independence of atmospheric changes, in respect to the quantity and quality of the gas; and (where the water-wheel is used) entirely automatic action.

The machine exhibited was tested by the judges to its full capacity of forty lights, as rated by the exhibitors, and was found to be

equal to this duty. The following test was made to determine the promptness with which the machine would adjust itself to extremes of consumption.

Provision was made by which all the lights, or a number of them, could be extinguished simultaneously, leaving only one, or a predetermined number, burning. The experiment gave the following results:

	Observed Pressure.
With 5 lights on,	1·5 inches.
With 38 lights on (lighted as quickly as possible),	·6 inch.
With 1 light on (20 suddenly put out, and 17 as quickly as possible),	1·8 inches.
Total variation of pressure from (approximate) maximum to minimum capacity of machine,	1·2 inches.

The single remaining light exhibited considerable blowing, persisting for several minutes, but was not blown out.

Photometric examination of the gas delivered by the Victor machine gave the following result, as the mean of twenty observations.

FIRST SERIES.

Gas consumption per hour,	5 cubic feet.
Ignition pressure,	1 inch.
Burner used, the U. S. Standard.	
Illuminating value,	22·33 candles.

SECOND SERIES.

Gas consumption,	5 cubic feet.
Ignition pressure,	1·5 inches.
Burner used, Gefrorer's (furnished by R. & H.)	
Illuminating value,	19·5 candles.

(*Observers: Wahl and Ward.*)

The judges admit the claims of the makers so far as they relate to the simplicity of design and automatic action of the "Victor" machine, but cannot admit the claim that the quality and quantity of gas made will be entirely independent of atmospheric changes.

With the carburetting-chamber buried in the ground at a distance from the house to be lighted, and provided with a safety-valve to make it secure against back-pressure, the machine would appear to meet the requirements on the score of safety which the insurance interest demands.

The regulating mechanism, *i. e.*, the floating air-chamber with

automatic water-supply is ingenious, and where so situated as to be free from liability to freezing, may be commended as a simple and useful device.

THE PENNSYLVANIA GLOBE GAS LIGHT COMPANY, PHILADELPHIA.

The Royal Gas Machine.—This machine comprises an air-pump, which is the usual device of a meter-wheel actuated by weights and pulleys; a carburetter, in which the air is made to pass over fibrous material saturated with gasolene, and from which it passes to the burners.

To meet the objection urged against the usual form of carburetters, in which the entire supply of gasolene is placed, viz., that the more volatile portions will first pass off, leaving the less volatile portions behind, thus yielding a variable light, at first very rich and becoming gradually impoverished—the makers have devised what they term an automatic feed, the object of which is to feed gasolene to the carburetter from a storage tank, in exact proportion to the consumption of gas. This device consists substantially of a box situated conveniently with reference to the storage tank and carburetter, and provided with a "float"-valve, by which the quantity of gasolene passing into the latter is automatically regulated by the consumption of gas at the burners. In other respects, the machine resembles those in common use.

The machine exhibited was tested by the judges to its full capacity (twenty-five lights) as rated, and was found to be equal to this duty.

The makers claim "simplicity of construction and operation, durability, safety and general utility."

To test the promptness of regulation of this machine under sudden and extreme variations of consumption, and the photometric value of the gas, the judges submitted it to the same tests as described in the case of the "Victor," and with the following results:

	Observed Pressure.
With 25 lights on	1·5 inches.
With 1 light on (24 quickly put out), ...	1·8 inches.
Total variation of pressure from maximum to minimum capacity of machine, ...	·3 inch.

The single remaining light flared slightly, but in a few minutes became steady.

PHOTOMETRIC RECORD.

Results in the mean of 20 observations:

Ignition pressure,	1 inch.
Consumption,	5 cubic feet.
Burner (*Standard*).	
Illuminating value,	19·65 candles.

The makers state that they place the carburetter (and storage-tank) and feed-box, six or more feet underground, and at least fifty feet from the building to be lighted.

The judges are favorably impressed with the regulating qualities of the machine, and with the general excellence of its workmanship. They regard the automatic feed, which is the only novelty which is claimed for it, as a meritorious feature and worthy of commendation, since, in their judgment, it will permit of the production of gas of more uniform quality and will act as a safety-valve against the accident of excessive back-pressure. Substantially similar devices, however, for accomplishing the same objects are and have been long in use. The only objection that has been found against them, is the liabilty of flooding the carburetter in the event of the valve from any cause becoming inoperative.

The use of a wire-gauze strainer over the inlet orifice, to prevent the entrance of any obstructing material from the storage-tank—which is adopted by this company—the judges regard as a reasonable precaution against accident from this cause.

JAS. P. WOOD & CO., PHILADELPHIA.

The Globe Gas Generator.—This is a so-called "caloric" machine (*i. e.*, one in which the gasolene is vaporized with the aid of heat) as distinguished from the so-called "air-blowing" machine previously described. It is operated by steam, where this is available, or, by a system of hot-water circulation, the heat being supplied by means of gas taken from the machine itself.

It comprises a storage-tank for the gasolene; a -shaped pipe communicating therewith, one leg of which containing liquid gasolene, furnishes a constant hydrostatic column of sufficient pressure for the continuous action of the machine, while the other leg contains a vapor column of the same pressure; a gas-holder communicating with the vapor column and provided with suitable valves for automatically admitting gasolene vapor and air in predetermined proportions, and in quantity regulated by the consump-

tion; and a system of steam or hot-water circulation for effecting the artificial vaporization of the gasolene.

The machine embodies a number of novel features, of which the following are noteworthy:

A method of securing an operating force for the machine without the use of a pump, and without elevating the storage-tank above the level of the machine. This is accomplished by placing the storage-tank (containing the gasolene) below the level of the machine, and running a pipe from the bottom of this tank, downward about nine feet, then turning it upward, surrounding this part with a heating-jacket, and attaching the upper extremity to an injecting mechanism connected with the gas-holder. This construction provides a hydrostatic column (of approximately three-pounds pressure) of gasolene, which, being in communication with a vapor column in the other leg of the bent pipe, gives to this a constant pressure, and supplies the force for actuating the machine.

A pipe leads from the top of the storage-tank to the lowest point of the gas-main, for the purpose of giving constant automatic drainage, and of establishing and maintaining equilibrium between the gasometer and the tank.

The pressure of the vapor column is utilized for regulating the heat required for vaporizing the gasolene. This is effected by a valve in the gas supply-pipe leading to the heating-burner, which is opened and closed by the tension of the vapor.

A system of heating and circulating water below the source of heat by placing one branch of the circulating-pipe within the other, thus keeping outflowing and return currents separated, and the adjustment of the operating mechanism in such a manner that the vapor-valve shall be fully opened and closed where the consumption is limited to a few burners, and partly opened and regulated where many burners are in use.

The actuating mechanism consists of two valves—a vapor valve, moving vertically, and an air-valve, which can be adjusted to deliver any desired proportion of air with the gasolene vapor entering the holder.

The operation of the machine is briefly as follows:

The gasolene vapor is generated (when the machine is in action) in the jacketed pipe before described, and forced through a vapor-valve into what may be called an injector, placed near the bottom

of the gas-holder and communicating therewith. This vapor-valve is opened automatically by the descent of the gasometer to a certain point, through the action of a trip movement connected therewith by a pivoted lever; when the gasometer rises again to a certain point the vapor-valve is closed. Connected with this injector is an air-valve, which admits air by inspiration as the vapor is injected, the proportions of the mixture being regulated by means of an adjustable screw-weight.

The heating apparatus is a coil of pipe, with leading and return branches above described, the heat being supplied by gas furnished from the machine, and automatically regulated by the tension of the vapor.

In practice, the makers explain that they place their machine in a small enclosure in the open air, and the heater near this enclosure, with an open-air space between them, to prevent dangerous accumulation of gas by leakage, should any occur.

They claim for this machine, as compared with other "caloric" machines, simplicity, in dispensing with the pumping of air, water or gasolene; doing away with the necessity of elevating either water or gasolene to secure an initial actuating pressure; positive safety against flooding the machine with gasolene should the heat be accidentally stopped; and doing away with the necessity of handling and providing for the removal of condensed gasolene after it is once placed in the tank.

They claim, as compared with the "air-blowing" machines: The maintenance of a positively uniform quality of gas under all circumstances, the proportions of air and vapor delivered remaining constant, no matter to what extent the amount of gas produced may vary.

The machine was submitted to the same tests as the others, before described, to determine its regulating qualities, and with the following result: The machine was rated as a 100-light machine, and was found to be capable of performing that duty.

	Observed Pressure.
With 30 lights on, ...	0·9 inch.
" 50 " " (loss not appreciable.)	
" 75 " "	0·7 "
" 100 " "	0·65 "
Turning off 50 lights suddenly, ...	0·9 "

A single gas-jet left burning exhibited little or no blowing.

PHOTOMETRIC TESTS.

Gas consumption per hour, ...	5 cubic feet.
Ignition pressure, ...	1 inch.
Burner used (*U. S. Standard*).	
Illuminating value, ...	23·83 candles.

The judges believe that the claims of the makers above enumerated are substantially correct. The mechanical details are very ingeniously contrived, and the features on which the automatic action of the machine depends appear to be effectual and reliable. The avoidance of the use of an elevated storage-tank, and of mechanical means of securing an initial actuating pressure, and the method adopted for securing and maintaining such pressure, are ingenious and commendable, inasmuch as they render the accidental flooding of the machine, with gasolene, impossible. The compactness of the machine also is noteworthy.

NOTE.—The photometric observations in the foregoing were made on a 100-inch Bunsen photometer, having a candle balance and decimal meter at the appropriate ends. A sperm-candle burning two grains of hydro-carbon a minute was used as the standard of comparison. The mode of procedure consisted in (1), determining the quantity of gas consumed in one hour; (2), the water-pressure at the meter and at point of ignition; (3), to determine the consumption of the candle. Five-minute duration tests were made to enable the observer to take ten photometric observations, and also to determine the consumption of the candle. The mean of the ten observations, corrected by the proper reduction factor when it was needed, was taken as the correct result. From this result could be obtained the candle-power per cubic foot of gas burned.

(*To be continued.*)

SLIGHT VARIABILITY IN VELOCITY OF LIGHT.—Cornu calls attention to the confirmation which Michelson and Morley's recent measurement of the velocity of light along a swift current of air or water gives to Fizeau's result of 1851. The air-current does not sensibly affect the velocity of light; the water-current affects it by about half of the velocity of the stream. This agrees with Fresnel's surmise, based on an observation of Arago, that the absolute velocity of light is altered by $\frac{n^2-1}{n^2}$ of the current's velocity, n being the index of refraction; and it may indicate that the ether does not even partially partake of the current's flow.—*Comptes Rendus, May 31, 1886.*

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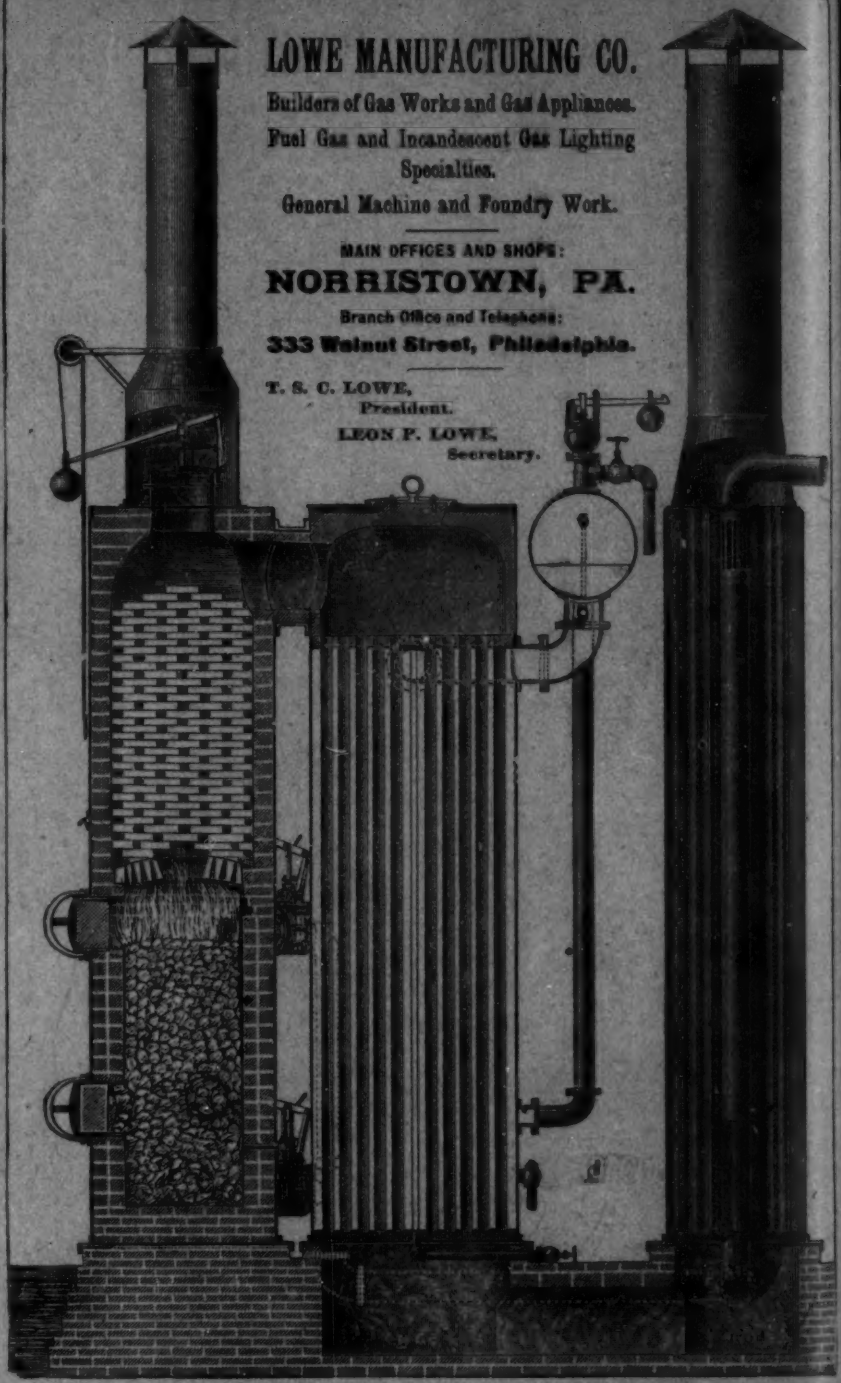

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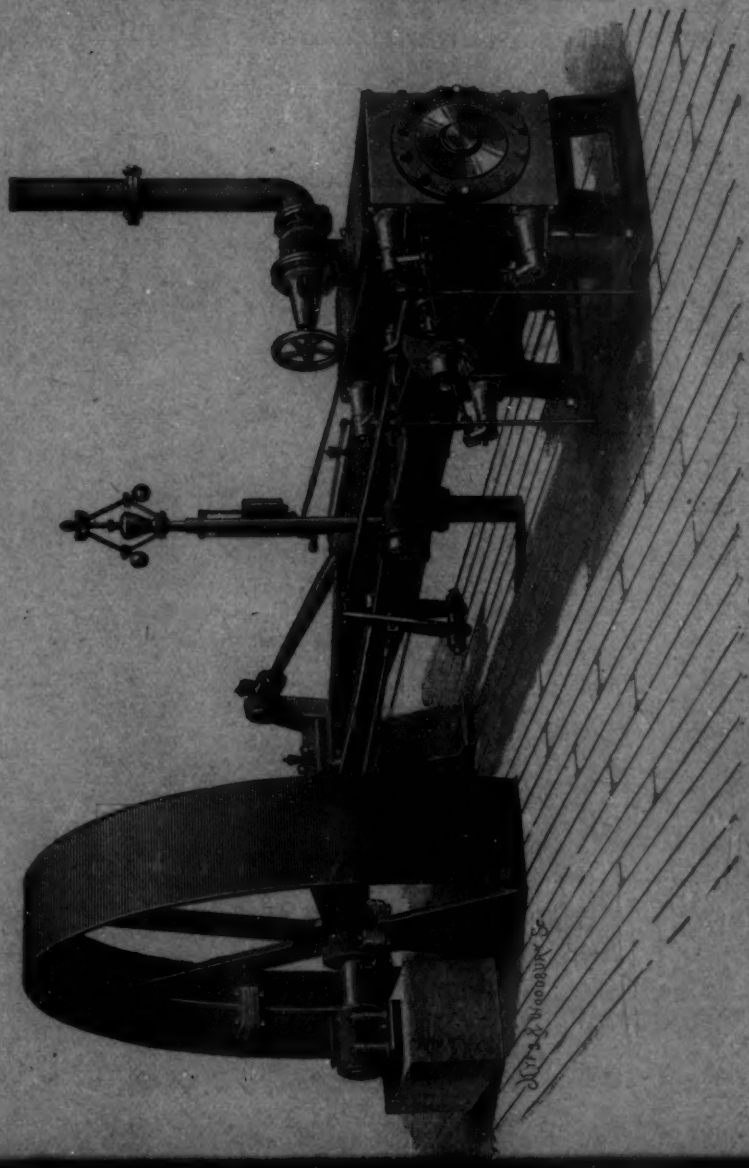

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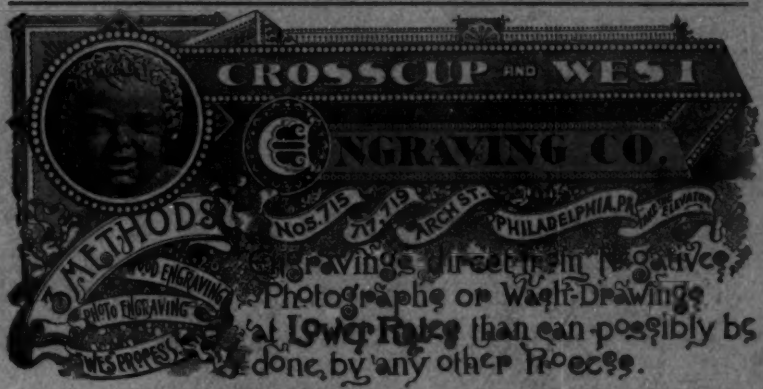

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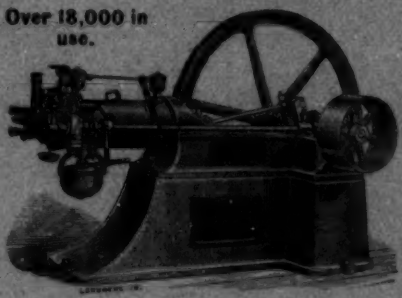

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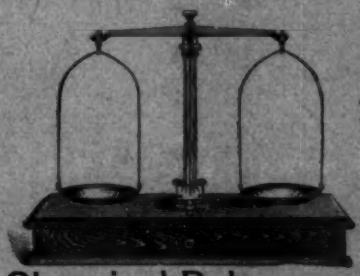

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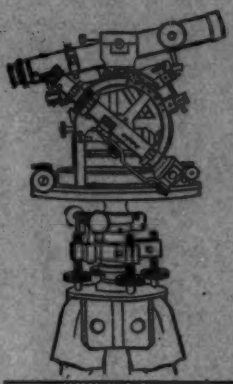

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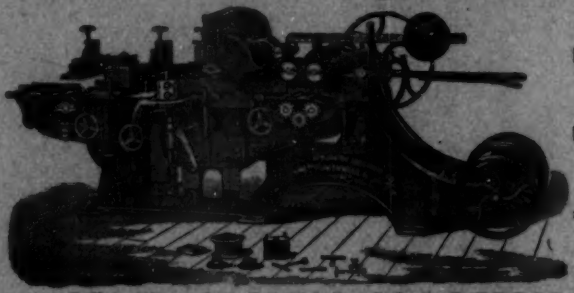

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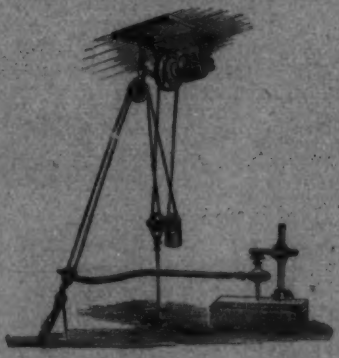

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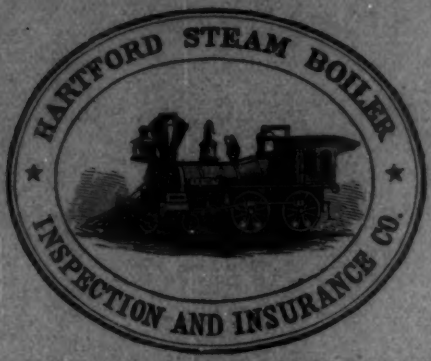

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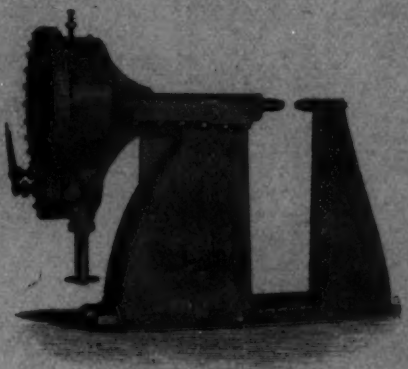

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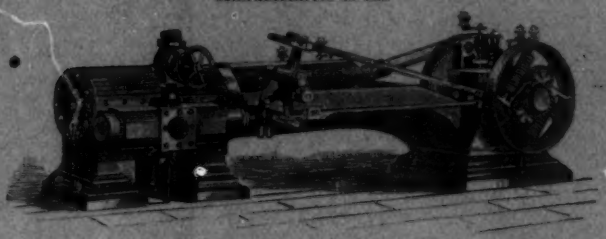

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Third Series. *DECEMBER, 1886.* VOL. XCII.—No. 6

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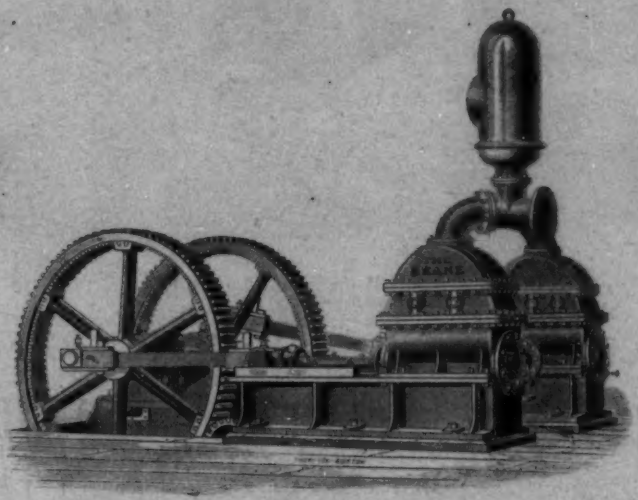

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